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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

On the Mineral Water of the Baths of Nevis, in the West Indies; in a Letter addressed to Professor JAMESON, *by* JOHN DAVY, M.D., F.R.S, London and Edinburgh, Inspector-General of Army Hospitals.

MY DEAR SIR,—In a visit which I made last year to Nevis, I paid some attention to the mineral water of which there is a spring at the baths of that island; and through the kindness of James Davoren, Esq., of Nevis, I obtained portions of the water for examination on my return to Barbadoes.

As, to the best of my knowledge, this water has not hitherto been analyzed;—as its effects, when drank, are decidedly beneficial in many instances of derangements of function of the stomach and intestinal canal;—and, more especially, as some of the results I have obtained in the trials made on it bear on points interesting in relation to geology, and in which I know you take a lively interest, I would hope that a short account of the baths (avoiding minute detail, as unsuitable to the occasion), and of the analysis of the mineral water, with a few incidental remarks and inferences, may not be unacceptable to you and to the readers of the Philosophical Journal.

The Baths of Nevis are pleasantly situated on a rising ground, reputed healthy, about half a mile from the sea-shore. They are two in number. Each is sufficiently large to admit of swimming in it. One is warm, the other tepid; the former was 98° of Fahr. when I tried it; the latter 88°. Adjoining them is a well, or rather a little cistern, the water of which is used for drinking. This is the mineral water of Nevis. I found its temperature 110·5° immediately under the mouth of the pipe, through which it flows into the cistern. It was perfectly clear, and free from any appearance of effervescence or of disengagement of air. It had a just perceptible taste, not to me disagreeable, and by some persons liked. The water with which the baths are supplied, and this the drinking water, are, I believe, from the same source, and of the same quality; the temperature of each depending on circumstances of an accidental nature: I say, I believe, for it may be otherwise, as I have examined only the water of highest temperature, that used for drinking.

The specific gravity of this water reduced to the temperature of 80°, I have ascertained to be 10·019. Though not sparkling, this water is nearly saturated with carbonic acid, and contains its own volume of the gas.

From 44 cubic inches, that is about 11·120 grains of the water, there were obtained, besides carbonic acid, 1·8 grains of saline matter, readily soluble in water, chiefly bicarbonate of potash, with a trace of muriate of magnesia, 1·3 of carbonate of lime, ·77 grain of carbonate of magnesia, 1·5 of silica, a trace of phosphate of lime, of sulphate of lime, and of vegetable matter. I sought in vain to detect in it either iodine or bromine.

The vegetable matter, a kind of mucor, was found in bottles of the water that had been kept many months, though confined with glass stoppers. It appeared in the form of minute delicate fibres, partly black, partly green, and when spread out colourless. Under the microscope it was seen as an irregular tissue of nucleated fibres, of an average width of about $\frac{1}{4000}$ of an inch. Occasionally after long keeping, the water had a smell of sulphuretted hydrogen. This it may have derived from the decomposition by the vegetable matter of a little sulphuric acid in the minute portion of sulphate of lime present. In one instance that I perceived the odour of this gas, the water was confined by a common cork; and, in that instance, no sulphuric acid, after the sulphuretted hydrogen was perceptible, could be detected in the water by the test of muriate of barytes.

I need hardly remark, that the carbonate of lime which the water contains is deposited on the escape, or expulsion, or neutralization of the free carbonic acid. It is worthy of remark, that a little silica, with a trace of phosphate of lime, is deposited at the same time. It is also worthy of remark, that when the bicarbonate of potash (a rare ingredient in a mineral water), was obtained by evaporation to dryness, thus insuring the removal of the excess of carbonic acid,—on its being redissolved, almost the whole of the silica remained, forming a strongly adhering incrustation of a dead white colour.

These, and other facts on the direct action of carbonic acid on silica, some of which I have noticed in a paper lately communicated to the Royal Society of London, would seem to prove, that carbonic acid is capable of dissolving silica. As this is a matter of importance, it may perhaps be right to be more particular in describing some of the results. I shall notice two in particular.

A bottle with a glass stopper, two-thirds full of Nevis water, after having been kept many months, was carefully observed. Corresponding to the surface margin of the water, a line of deposit was found of white matter without lustre, mixed with minute crystals, with which the inside of the vessel was studded below. The water was next poured out, and the incrustation examined. It consisted of silica and carbonate of lime chiefly; the former remaining after the action of dilute nitric acid on it, and washing with water. It was best seen when dry, for when wet it was scarcely visible. Under a high magnifying power, it appeared in part crystalline, but more generally in delicate laminæ.

The water poured out of the bottle, on admixture with ammonia, became turbid. After standing some time a deposition took place from it on the sides of the glass vessel, and this deposit also had the properties of a mixture of carbonate of lime and of silica; thus, after having been dried, the carbonate of lime, on the application of an acid and water was removed, whilst the silica in the form of a most delicate incrustation, remained, having resisted the action of the nitric acid and water.

These two results (not to revert to others) seem to shew, and it appears to me in a satisfactory manner, that silica may be, and is kept in solution by carbonic acid. If this be admitted, may it not aid in the explanation of much that is obscure in regard to the structure of certain rocky strata, especially of aggregates, in which the cementing principle appears to be silica? May it not also aid us in explaining the manner in which certain fossils are formed, such as are, as it were, casts, the petrifying matter of which is silica?

Hitherto a fixed alkali has been received as the solvent of silica, in mineral waters. That it is a solvent in its caustic state, I apprehend, must be granted;—but I am not aware of any facts or analogies tending to prove that it is also such a solvent when saturated with carbonic acid. And as, in every instance in which I have had an opportunity of examining waters containing silica held in solution, I have found carbonic acid, it seems to me natural to infer, that it with water is the solvent. As regards this view, it is desirable that the waters, most remarkably siliceous, should be re-examined; especially the water of the Great Geyser in Iceland, the petrifying effect of which, as it falls on the plants in the neighbourhood of the fountain is so remarkable, and so obscure at present, but which would cease to be so should it be proved that the deposition of silica in this instance is connected not only with a great and sudden reduction of temperature, but also with the rapid escape of carbonic acid,—the generation and evolution of which may perhaps be concerned in the production of some of the peculiar phenomena of this extraordinary spring.

The island of Nevis, like Iceland, is volcanic. Some of its rocks are highly crystalline, and abound in felspar, or in what appears to be felspar. Clay, too, is abundant in the island. Probably the potash which occurs in the mineral water is derived from the decomposition of felspar, or of analogous minerals, aided by heat, which expels carbonic acid from compounds containing it; and this acting on the silica liberated from the decomposing minerals, may, with water, effect its solution:—I say *liberated*, because we know that certain substances, perhaps all, then most readily unite with others for which they have a feeble affinity, when in that state, which has been designated their nascent state, in which we may suppose that they are in a condition of the subtlest division,—perhaps no two particles yet cohering. Not only are crystalline rocks and clays found in Nevis, but also well formed crystals of quartz of perfect transparency, which, under the name of diamonds, are in no small estimation amongst the inhabitants for ornamental purposes, after having been cut and polished. They are found, I was informed, loose in the beds of torrents; but whether they are of aqueous or igneous origin, I could learn nothing on which to found even conjecture.

I have alluded to the beneficial effects of the mineral water of Nevis, used as a drink. Considering its composition, such an effect, is what perhaps might be expected. I may also notice, that the baths have proved very beneficial in obstinate cases of chronic rheumatism. Our great naval hero, Lord Nelson, it is likely used them; for, when in infirm health, we are informed, he recruited it in this island, residing at a spot about two miles distant from the baths, in a house the property of the uncle of Mrs Nesbit, a native of Nevis, whom he married at this time.

For invalids, to whom these waters are likely to be useful, these baths hold out many recommendations. There is a good lodging-house adjoining them, built specially for the accommodation of invalids, by a philanthropic individual;—they are about half-a-mile only from James Town, where medical advice can be obtained, and where the steamers of the Royal West India Mail Company touch twice a-month.

In a former communication which appeared in the Philosophical Journal, I took occasion to notice defects of ventilation in the vessels of this Company. I regret to learn that these defects continue; and that others are added to them, arising out of want of proper attention to the comforts of passengers. When the winter climate of the West Indies is considered, compared with our home winter climate, and the probable benefit that a numerous class of persons in feeble health might derive from a voyage to the West Indies, and a sojourn here for two or three of the coolest months, any obstacle in the way is surely to be lamented, and especially obstacles so serious as those alluded to, as regards invalids. In sailing ships, West Indiamen, attention is now being paid to ventilation:—improved methods have been introduced in many of them; and on this account, and on account of the convenience and comforts they afford, they are preferred by some passengers, notwithstanding they are commonly nearly twice as long in making the homeward voyage. The competition should have the attention of the Managing Committee, or the Directors of the Company, who, could they secure comfort as well as speed, in the transit, might rest secure of their vessels having the preference, at least in the great majority of instances.

I am sure your humanity will find an excuse for these latter remarks, which I offer with the hope that they may meet the eye of some influential person belonging to the Company, well disposed to have the West India Mail Steamers made as comfortable and as wholesome as possible for passengers, and as profitable as possible to the proprietors,—a union of interests much to be desired. I remain, my dear Sir, with much esteem, faithfully yours,

J. DAVY.

TO PROFESSOR JAMESON, Edinburgh.

New Considerations on the Palæontology of Auvergne.

By A. POMEL.

The numerous fossil bones contained in the formations of Auvergne have already been either described or mentioned by many authors; but they have too often been contented with superficial and incomplete researches. Some have neglected to study carefully the different deposits in which they lie, and others have inconsiderately established ill-defined species on mere individual variations. M. De Blainville even, in his beautiful and classical *Osteographie,* has often allowed his mind to be pre-occupied with theoretical notions which he still maintains, almost alone against all other palæontologists, and he has not given the necessary care to geological indications. Now, it is only by introducing an extreme degree of precision into the zoological analysis of characters, and the geological study of formations, that palæontology can find that solid and lasting foundation of which the precipitation of some individuals has sometimes deprived her.

M. Pomel, in the two memoirs with which we are acquainted only by an extract, and which we hope will be laid immediately before the public *in extenso,* shews that, in the deposits of the basin of the Allier, there are three different ages, which prove that three very distinct faunas have succeeded each other since the commencement of the tertiary epoch, in this which corresponds at present to Auvergne. The oldest of these faunas belongs to the *miocene* epoch, and is contemporary with that of the *faluns* of Touraine, the sandstones of Fontainbleau, &c. It is characterised by numerous lost genera.

The second fauna, which M. Pomel considers more particularly in his first memoir, belongs to the *pliocene* period. The organic remains of the animals which compose it have been preserved in the most ancient deposits of the pumice conglomerates, chiefly at the foot of Mount Perrier. The genera, for the most part, belong to the existing fauna, but the species are all extinct. We may mention, in particular, the *Canis megamastoides*, the *Ursidæ, Mustela lutroides, Lutra Bravardi,* the Hyæna of Mount Perrier, the genus *Stenodon*, a *Mastodonte*, a species of Rhinoceros, tall and slender, a Tapir, numerous species of Deers very different from those now living, small Oxen, high on their legs, &c.

The third fauna, which has been too often confounded with the second, belongs entirely to the diluvian epoch. The bones are preserved among the debris at the base or the sides of hills, under lavas, in fissures, in the travertins and mud of certain grottoes. The deposits containing them are scattered over a multitude of localities, chiefly in the valley of Limagne. The animals composing this fauna belong, in great numbers, to the existing species. Dogs, martins, otters, &c., cannot be distinguished from those now living; cats replace the stenodons; there is no mastodons, and the elephant supplies its place; the rhinoceros is the Siberian species (*R. tichorhinus*). The oxen exhibit the heavy forms of the present kinds; the horse and hippopotamus supply the place of the lost genera. The Deers greatly resemble those of our own times, &c.

M. Bravard (*Bull. Soc. Geol.* iii., 197,) has attempted to distinguish two distinct faunas in this epoch, and he separates the *elephantine* fauna contained in the more recently formed lands of Champeix, Veneix, Tour de Boulade, &c., from the *diluvian* fauna contained in the caverns or fissures of Coudes, and the sub-volcanic alluviums of Neschers. The investigations of M. Pomel seem to prove that there are not grounds for maintaining this division.

The resemblance between the species of M. Pomel's diluvian epoch and those of the existing world, seem to me to furnish a new and powerful argument in favour of the opinion which I expressed, (*Traité Elem. de Paleontologie*, tom. i., note B,) that the diluvian epoch is connected without interruption with the modern epoch. It was not, therefore, without surprise that I saw M. Pomel conclude by saying that it is impossible for him to admit my opinion on this subject. I am certain that if this skilful geologist again examines the question, he will find, according to his own observations, 1*st*, That the majority of the species of the diluvian epoch have not been destroyed at the end of that epoch; 2*dly*, That there has not been a sudden appearance of an entirely new fauna at the commencement of the modern epoch; 3*dly*, That between the diluvian and modern epoch there has been no event which has acted on organisation in the same manner as those which have separated the tertiary epoch from the diluvian epoch, the cretaceous epoch from the tertiary, &c.; 4*thly*, That consequently the diluvian and modern epochs are not separated by characters similar to those which separate the other geological epochs. M. Pomel may the more readily admit these conclusions, since he insists, with reason, on the fact that the gravels of the diluvian epoch have not been deposited by a decisive and instantaneous phenomenon, but rather by a series "of small successive local inundations which could not operate at the same time, and that there was a long diluvian epoch which might more properly be called alluvial, since the latter name has a more general acceptation, and may be applied to diluvium, which is only an exaggerated paroxysm of this great period."

There is another fact in M. Pomel's memoir, which ought to be of great interest to geologists at a time when the attention is so actively directed to all that relates to glaciers. During the diluvian epoch, that is to say, during the period when the *glacialists* suppose that the extension of the glaciers round the Alps was much greater than it now is, animals lived in the centre of France which are now banished to the coldest regions of our hemisphere. The reindeer, the lagomys, the spermophilus, rarely the glutton, &c., seem by their presence to justify the hypothesis that the climate of central Europe has been for a time colder than at present. It is true, adds M. Pomel, that we have not yet found any geological traces of the existence of glaciers in Auvergne, although this country presents very elevated mountain summits.

The second memoir is devoted to the study of the miocene epoch in the basin of the Allier. After giving a geological sketch of the constitution of the deposits in the valley, M. Pomel considers the vertebrate animals composing the fauna of this remarkable period. Among the carnivora he particularly notices *Amphicyon minor*, a dog with a short head, which appears to have wanted the second lower tubercular; two civets (*Viverra antiqua*, Blainv. and *primæva*, Pom.); the *Plesictis genettoides* (*Mustela plesictis*, Delaizer and De Parieu), which forms a genus intermediate between the martins and *genettes; Lutra Valetoni*, (Geoff. St Hil.); *Megantherceon brevidens*, a remarkable species belonging to the tribe of cats, and which ought to be united to some other species of Europe and America, in order to constitute a genus which has already received the names of Meganthereon, Stenodon, Machairodus, and Trepanodon; and a *Pterodon*, an anomalous genus probably belonging to the subclass Didelphi.

The Gnawers are numerous, but as yet little known. M. Pomel mentions *Steneofiber*, *Archæomys*, and the *Rats*.

In the division of Pachyderms, we find the *Dinotherium giganteum;* a tapir of small size and slender limbs (*Tapir Poi-*

rieri); some *Rhinoceros*, not yet well determined; many *Anthracotherium;* and the genus *Cainotherium*, Bravard, (*Oplotherium*, Delaiz.), which connects the Anoplotherium with the Ruminants.

The Ruminants present many species of *Amphitragulus*, a new genus of the family Moschus, and which, in its dentition, has relations on the one hand with the Llamas, and, on the other, with the Anoplotherium. Some bones not well known and described under the name of *Dremotherium*, are likewise referred to in the order of ruminants.

The Birds have not yet been determined. The bones of of palmipedes, waders, and birds of prey, have been found.

Among the Reptiles, M. Pomel mentions a land tortoise (*Testudo gigantea*), two emydes, an emysaurus (*E. Meilheuratiæ*), a trionyx, a crocodile (*C. Ratelii*), a *Dracænosaurus*, an animal nearly related to the dragon in its dentition, and one or two frogs.

Fishes likewise exist, but the fragments have as yet been imperfectly studied.—*Professor Pictet, Bib. Univer. de Geneve, 15th September* 1846.

On the Volcanoes of the Moon. By JAMES D. DANA. Extracted from the American Journal of Science, vol. ii., Second Series.*

The surface of the moon affords a most interesting subject for the study of the geologist. Though at a distance of many thousand miles, the telescope exhibits to us its structure with wonderful distinctness; and already, as a learned astronomer has observed, we are better acquainted with the actual heights of its mountains, than with those of our own planet.† Having an atmosphere of extreme rarity‡ (if any) and never obscured by clouds, its features are wholly open to view, and the eye, aided with glasses, may wander over its rugged crags, survey its craters, its Alps and its Apennines, from their bases to their summits. Neither are there any sedimentary deposits, soil or vegetation,—for there can be none without water,—and the igneous surface, therefore, is still its own naked self, exhibiting the results of igneous action in their simple grandeur, unaltered and uncomplicated by any attending operations. We may hope, therefore, to find some profit in contemplating for a few moments this land of the skies: and although we may not look for very speedy "annexation," we may possibly gather some facts and ideas which the decree of Truth will annex to the domain of Science.

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* Read before the Association of American Geologists and Naturalists, Sept. 1846.

† M. Arago, Annuaire des Longitudes, pour l'an 1842, 2d ed., Paris, 1842.—P. 526, in an article on the Lunar Volcanoes, Arago says :—" Il est remarquable que grace au zèle et à l'exactitude d'Hevelius on ait connu la hauteur des montagnes de la Lune beaucoup plus tôt que la hauteur des montagues de la Terre."

‡ The evidence in favour of the existence of an atmosphere and of water in the moon, hitherto obtained, has not been deemed satisfactory. Herschel, at an

The moon, as we all know, has been minutely studied in a physical point of view, and already some important geological conclusions have been drawn from the facts it presents. The

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eclipse, Sept. 5, 1793, observed the sharp horn of the limb of the moon, and says that it seemed perfectly regular; and that a deviation of a single second by the refraction of the solar light in the moon's atmosphere would not have escaped him. Phil. Trans., 1794, p. 39.—As stated in Beer and Mädler (p. 133), Schröter calculated the density of the supposed atmosphere to be one twenty-eighth the density of our own atmosphere; and Melanderhjelm demonstrated that the moon's atmosphere, judging from that of the earth, should have one thirty-sixth the density of our own atmosphere. But the above-mentioned authors say that we have yet to prove that the moon has any atmosphere, adding, that it must be very much more rare than the rarest gas on earth. They observe also, that supposing our atmosphere to extend through space, its density half way to the moon, according to the Mariottian law of decrease, would be expressed by the fraction $\frac{1}{1000000}$, the denominator extending to ten thousand zeros. The singular observation occasionally made, that during the passage of the moon over a star, the star appears visible in front of the edge of the moon, before disappearing, may possibly indicate an extremely thin atmosphere or surface vapours: but it has been attributed, with much appearance of reason (Rep. Brit. Assoc., 1845, p. 5), to diffraction.

The absence of any bodies of water on the moon is placed beyond doubt, both by actual telescopic examination, and by inference from the absence of clouds. There are no streams, lakes, or seas. An eminent astronomer has remarked, that the heat of the surface exposed to the sun would occasion a transfer of any water the moon might contain to its dark side, and that there may be frosts in this part, and perhaps running water near the margin of the illuminated portion. But in such a case, would not clouds appear about the margin at times in telescopic views?

altitudes of its mountains were first estimated by Galileo,* and afterwards were mathematically calculated by Hevelius† and Riccioli. Sir William Herschel continued the investigations, and reported the probable activity of three of its volcanic mountains.‡ Mayer, Huth, Harding, and Schröter,§ and more lately Gruithuisen and W. G. Lohrmann,‖ are other prominent names among those who have added largely to our knowledge of the moon's surface. More recently still, MM. Beer and Mädler have pursued this science of Selenography with wonderful perseverance and labour, and have given corrected results of all previous calculations, with magnificent maps of the moon's topography.¶ 1095 heights were carefully measured by them, and their features, to a great degree of accuracy, ascertained. These maps have afforded M. Elie de Beaumont some deductions alleged as supporting certain geological theories. James Nasmyth, Esq., in the Transactions of the Royal Astronomical Society for the present year, has published important observations on the features of the moon's mountains, and traced out their volcanic character.** A very valuable memoir, on the same topics, has been presented within the current year to the Institute at Paris, by M. Rozet, in which the moon is shewn to have been a globe in complete fusion, which has slowly cooled; and its peculiarities are dwelt upon as an exhibition, in many respects, of the former state of our own planet.*

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* In the article referred to in the Annuaire des Longitudes, (p. 522), Arago states that Clearchus, on the authority of Plutarch, described the moon as smooth and lustrous like a mirror. Democritus attributed the spots to inequalities of surface. Galileo first observed the lunar mountains with his telescope in 1610, and estimated their height at one-twentieth of the diameter, giving 8800 metres for their altitude, which but little exceeds their actual height.

† J. Hevelius, Selenographia; fol., Gedani, 1647.

‡ Phil. Trans. for 1780, p. 507, Astronomical Observations relating to the Moon:—for 1787, p. 229, An Account of three Volcanoes in the Moon:—for 1794, p. 39, Account of some particulars observed during the late eclipse (in 1793) of the Sun.

§ J. H. Schröter, Selenotopographische Fragmente zur genauren Kenntniss der Mondfläche ihrer erlittenen Veränderungen und Atmosphäre; 2 vols, 4to, Göttingen, 1791 and 1802.—Gruithuisen, in Bode's Astron. Jahrb., 1825.

‖ Topographie der sichtbaren Mondoberfläche, von W. G. Lohrmann; 4to, Dresden und Leipzig, 1824.

¶ Allgemeine vergleichende Selenographie; mit besonderer Beziehung auf die von den Verfassern herausgegebene Mappa selenographica, von W. Beer und Dr J. G. Mädler; Berlin, 1837.

** Memoirs of the Royal Astronomical Society, vol. xv., 1846. On the Telescopic Appearance of the Moon, by James Nasmyth, Esq., p. 147.

In all the geological observations which have been hitherto made with regard to the moon, one important feature remains unsatisfactorily explained. I refer to the vast magnitude of its craters. It is not surprising that, in view of their stupendous size, many should have been incredulous as to their crater character, and preferred to designate them by some non-committal term, as circular ridges, or ring-mountains; nor that geologists, in general, have hardly ventured to acknowledge their belief in these lunar wonders. Imagine, if possible, in place of an ordinary crater, circular areas 50 to 150 miles in diameter, and 10,000 to 20,000 feet in depth. Such are many of the lunar craters; and they are crowded in great numbers over the larger part of its surface, varying from even a more capacious magnitude, down to those that measure but a few miles in breadth. It is not astonishing that there should be found much difficulty in reconciling their features with those of Vesuvius and Etna, hitherto received too generally as the types of volcanoes and volcanic action. The crater of Kilauea in the Hawaiian Islands is of a wholly different character, and I propose to present some illustrations which it affords, appealing to such general facts regarding it as are already well known. If I mistake not, it will be found to give a full interpretation of whatever has been considered mysterious in these lunar ring-mountains. After these illustrations, we may return again to earth, and apply the knowledge which we have derived abroad, in exemplifying the former geological history of our own planet.

We may first consider the general features of the moon's surface.

About two-thirds of the lunar hemisphere in view, comprising almost the whole of the southern half and the north-east quarter, are covered thickly with volcanic mountains.

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* Sur la Sélénologie, by M. Rozet, Comptes Rendus, 1846, xxii., 470.

Over a large part of the north-west quarter there is only here and there an elevation, and this comparative nudity extends a considerable distance southward across the equator.

The features of the surface may be distinguished as of five kinds, viz :—

1. The ring-mountains, which are broad truncated cones with immense circular craters. (See the following figures from Beer and Mädler.)
2. Conical mountains, nearly like ordinary volcanoes.
3. Linear or irregular ridges.
4. Large depressed areas, usually termed seas, but not supposed to contain water.
5. Broad pale streaks, of great length.
6. Narrow lines, supposed to be fissures.

Out of the 1095 heights measured by Beer and Mädler, *six* are above 20,000 feet in altitude, and *twenty-two* exceed 15,750 feet.

The broad truncated cones with large circular craters, are its most common elevations, and are among the loftiest. The pits, as we have remarked, are of all dimensions to 150 miles, and of various depths to near 25,000 feet. The crater *Baily* is 149½ statute miles in diameter; *Clavius* is 143⅓ miles; *Schickard* is 128 miles. 20 to 60 miles is the more common breadth. The depth of *Newton* is 23,833 English feet; of *Casatus* 22,822; of *Calippus* 22,209; of *Tycho* 20,181 feet.* The height above the surface exterior to the cone, is

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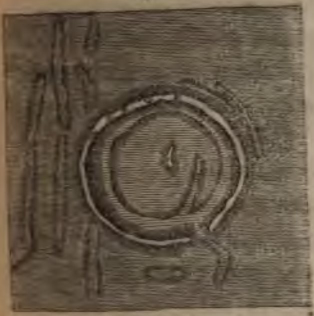
* We have stated that Galileo (note to page 336) made the altitude of the higher of the moon's mountain 8800 metres. Hevelius reduced their height to 5200 metres. Riccioli, as M. Arago states, increased Galileo's estimate, and his observations, as calculated by M. Keill, gave for the mountain St Catherine more than 14,000 metres. Herschel in 1780 (Phil. Trans. for 1780, p. 507; also for 1794, p. 40), reduced again the heights, concluding, from his observations, that the loftiest did not exceed a mile and a half. The latest investigations have restored them nearly to Galileo's first estimate.

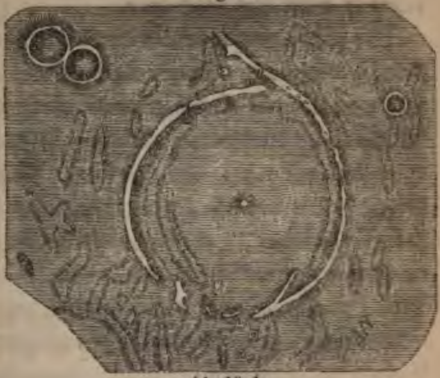
We state, for the information of some who have not paid attention to the subject, that these heights are determined, either from the shadows of the peaks on the central plain of a crater or the exterior surface, or by noting the position of a summit when it first becomes illuminated, and calculating therefrom; the higher the peak, the longer will be the shadow, and also, the sooner its top will be tipped with light. Should it hereafter be established that the moon has an atmosphere, it must be too slight to affect appreciably the altitudes de-said by Beer and Mädler to be often but one-half or one-third the height above the bottom of the crater; the outer slopes are generally steep, so that the margin appears like a raised rim around the pit.

Fig. 1.



Timocharis.

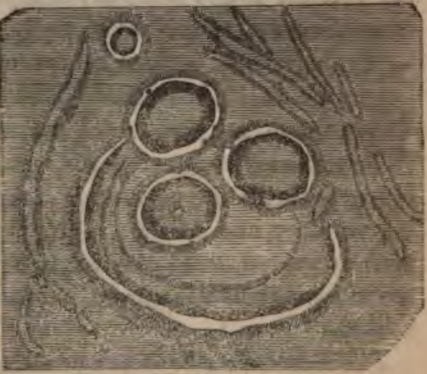
Fig. 2.



Abulfeda.

Mr Nasmyth figures one which is filled to its summit, and is tipped with a plain 40 miles in diameter;* looking, he says, as if "brim-full of molten lava," having cooled, probably, when thus filled.

Fig. 3.



Heinsius, seen a little obliquely.

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termined: with regard to the breadth of the craters, there can be no more doubt, than with respect to the diameter of the moon itself.

There are many who receive with scepticism the facts we have stated, or even deny where they know nothing. It is taking a high ground, to dispute with all the astronomers since Galileo, and one to which ignorance alone would presume. The best antidote we can propose to such presumption, is to take the first opportunity which offers, to look through a good telescope at the moon's surface, and examine its features for themselves. We predict that they will soon become conscious of a growing willingness to be humble learners of such men as Herschel and others who have made the moon their study.

* Mem. of the Roy. Astronom. Soc., xv., 152.

The largest craters are not contained in the highest mountains: on the contrary, they are of less altitude than those of medium size, and to a certain extent the height varies inversely with the diameter.

The pits are generally circular, and sometimes almost artificially regular. There are others which consist of two or more coalesced circular pits. In still others, especially the largest, the enclosing walls are broken into a series of ridges, sometimes with large openings like the break of an eruption; yet even then the irregular forms may generally be referred either to a single circle, to a combination of circles, or to the formation of successive ridges one within another. The bottom of the pits, though generally flat or nearly so, not unfrequently contains small cones, or ridge-like elevations; we call them small, though some are 5000 feet in height, for they are mere dots in the immense basin. Over the exterior slopes there are many lateral cones of the same *small* dimensions, and occasionally one as large as Etna may be distinguished, besides others of different sizes to a few hundred feet in breadth. There are also circular craters within the larger pits, which are of various dimensions.

The pointed cones or peaks, excepting those immediately connected with the pit-craters, are few in number. According to Beer and Mädler, *Dörfel*, the most elevated lunar peak measured, is 24,945 feet in height; it is situated in the lunar Apennines: *Huygens*, another peak, is 18,209 feet in altitude.

The mountain ridges are peculiar in being generally elongated elevations, or clusters of such elevations, without valleys intersecting their declivities, and thus very unlike the chains of our globe. As M. Rozet and others have remarked, there is no water on the moon to wear out valleys.

Many of the depressions called seas, of which the *Mare Serenitatis*, and *Mare Crisium*, are examples, vary in breadth to five or six hundred miles, and, notwithstanding their size, they are identical in character with the great pit-craters, their extent and less depth being their only characteristics. This view is suggested by M. Rozet, and their features early sustain it. They contain cones and circular areas ke the better defined pits.*

The light streaks alluded to form radiating lines around large cones, and especially about *Euler*, *Kepler*, *Copernicus*, and *Aristarchus.* They are from one to five hundred miles in length, and cross ridges and depressions, without interruption. They coalesce about the summit of *Kepler*, so that the whole surface appears nebulous.

The various pit-craters differ in shade of colour, or rather in the degree of light they reflect; and ten different degrees are distinguished in the work of Beer and Mädler. 1 to 3, he says, may be described as grey, 4 to 5 light grey, 6 to 7 white, 8 to 10 shining white. The so-called seas, though but slightly depressed, are sometimes very much lighter than the surrounding surface. In some instances, as these authors state, two pits, side by side, alike in size and features, so differ in brilliancy that one is wholly obscured in the full moon, while the other still shines: the two are seen together again as soon as their shadows reappear. The brightest craters are *Aristarchus*, *Werner*, *and Proclus. Aristarchus* is 7629 feet in depth. It has a point of greatest brilliancy, besides two or three separate circular spots remarkably light. *Werner* has a single brilliant point. *Proclus* has brilliant walls, yet it is dark at bottom.

Sir William Herschel published the first account of existing volcanic action in the moon. In a notice of three lunar volcanoes,† he says that two of them, on April 19, 1787, were either nearly extinct or about to break out, while the third was in actual eruption. April 20, he observed that the active one burned with greater violence; and he estimated that the fiery area was above three miles in diameter. All the adjacent parts of the crater seemed to be illuminated by the eruption.‡ The other two volcanoes, he says, resembled large pretty faint nebulæ, that are gradually much brighter at the middle, but no well defined luminous spot could be distinguished. Herschel alludes also to an eruption seen by him previously, in 1783.

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* The "seas," according to M. Rozet, have escarpments of 45 degrees, some of which are 400 metres in height. In the interior there are annular cavities, perfect rings in shape, the diameter of which attains sometimes to 100,000 metres.

† Phil. Trans. for 1787, p. 229.

‡ It is supposed that this crater was that called *Aristarchus*, or the *Mons Por-*

Such are the general facts, which call for explanation; to wit, the existence of circular pit craters, 5 to 150 miles in diameter, and 5000 to 24,000 feet in depth,—the great number of these pit craters, and their peculiar features,—the depressions of a similar character of still larger area,—and the various degrees of illumination of the craters. Well may the Vesuvian vulcanist look with doubt upon such vast gulfs; for he finds, in his well-known volcano, nothing parallel in kind or degree. The little dark hole at the top of his mountain has scarcely a single point of resemblance to the open walled areas of the moon.

But the case is different with Kilauea, to which we now direct our attention. We observe that the facts this crater presents are precisely the same in kind as those of the moon.

1. The crater is a large open pit, exceeding three miles in its longer diameter, and nearly a thousand feet deep.

2. It has clear bluff walls through a greater part of its circuit, with an inner ledge or plain at their base, raised 340 feet above the bottom.

3. The bottom is a plain of solid lavas, entirely open to day, which may be traversed with safety; over it there are pools of boiling lava in active ebullition, and one is more than a thousand feet in diameter. There are also cones at times from a few yards to 2000 or 3000 feet in diameter, and varying greatly in angle of inclination. The largest of these cones have a circular pit or crater at summit.

Compare these characters severally with the lunar craters, and an identity will be perceived even to the ledge that sur-

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phyrites of Hevelius. *Aristarchus* is described as apparently in action in 1821 by H. Kater, in the Philosophical Transactions for 1821, p. 130; also by Rev. M. Ward, at nearly the same time, in the Memoirs of the Royal Astronomical Society, i., 157; also the following year by Rev. Fearon Fallows, in the Philosophical Transactions for 1822, p. 237. Dr Olbers observed *Aristarchus* at the same time with Kater in 1821, and attributed the light to the reflection of the earth's light by its smooth rocks.

rounds the lower pit, and the various forms of the cones. A large number of the lunar craters have an inner circle either as a terrace or ridge. The ledge within *Timocharis* (fig. 1), is very similar to that of Kilauea, and is continued around the whole pit unbroken as in the Hawaiian crater. The other figures illustrate the same feature in different conditions. Some of them, too, contain circular areas with the rim scarcely elevated (fig. 2), and others raised into cones (fig. 3) : and so, in Kilauea, there are at times boiling lakes in the bottom plain, and other pools constitute the summits of cones which they themselves have formed. To appreciate the comparison, it must be remembered that the Hawaiian pit-crater is upwards of three miles in length, and averages nearly half this in breadth ; and that the largest boiling pool, though more than 1000 feet in diameter, is still a small spot in the extensive area. During times of greater activity, the whole pit is in every part lighted with the fiery lavas, overflowing at times from the numerous lakes, and jetted from the many cones.

The *circular* or slightly elliptical form of the moon's craters is also exemplified to perfection. For the lakes of Kilauea have this shape ; and although the pit itself is oblong, owing to its situation on a fissure, other large though extinct pit-craters of Mount Loa are quite as regularly circular in form. Some are twins ; that is, are made up of two or three coalescing circles.

We have chosen Kilauea for these illustrations, because it is now in action, and the features appealed to have been familiar to us, since the first publications of Admiral Byron. and Rev. Charles S. Stewart. I may add, that the facts are finely illustrated in the Narrative of the Exploring Expedition by Captain Wilkes,[*] and they will be farther detailed in the Expedition Geological Report on the Hawaiian Islands, now in course of preparation. The exact application of these facts, as far as regards general features, to the *summit* crater of Mount Loa, will be found fully sustained by the plans and views accompanying the Narrative.

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[*] See Narrative, vol. iv., p. 125, and the map of part of Hawaii, in vol. vi.

Whence all this close resemblance to the lunar craters, while other volcanoes are so different? It arises from the fact, that the action at Kilauea is simply *boiling*, owing to the extreme fluidity of the lavas. The gases or vapours which produce this appearance of active ebullition, escape freely in small bubbles with little commotion, like the jets over boiling water; while at Vesuvius, and other like cones, they collect in immense bubbles before they accumulate force enough to make their way through; and, consequently, the lavas in the latter case are ejected with so much violence, that they rise to a height often of many thousand feet and fall around in cinders. This action builds up the pointed mountain, while the simple boiling of Kilauea makes no cinders and no cinder cones. Still, although the lavas of this crater are not thrown to a great height, they may make cones of any angle, even by overflowing alone; especially by small or partial overflowings, which melt together, cooling at the same time rapidly. They thus sometimes raise a steep rim around a pool. This point has been well presented by M. C. Prevost,[*] and in another place we shall mention many facts in illustration of it.

If the fluidity of lavas, then, is sufficient for this active ebullition, we may have boiling going on over an area of an indefinite extent; for the size of a boiling lake can have no limits except such as may arise from a deficiency of heat. The size of the lunar craters is, therefore, no mystery. Neither is their circular form of difficult explanation; for a boiling pool necessarily, by its own action, extends itself circularly around its centre.[†] The combination of many circles, and the large sea-like areas, are as readily understood.[‡]

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[*] Bulletin de la Soc. Geol. de France, xi., 1839 à 1840, p. 183.

[†] Mr Nasmyth suggests that the quietness of the lunar atmosphere may account for the regularity of the circles.

[‡] M. Rozet observes, in his article referred to, that the moon's craters do not resemble those of our volcanoes: and he explains them by supposing, that during the cooling of the moon's surface there were *whirlpools* or circular flowings, which carried the scoria from the centre to the circumference, and thus accumulated the enclosing ridges. We see no cause for the existence of whirlpools; nor for such a result from vortical movements.

With so perfect a correspondence, and so satisfactory explanations by means of an appeal to facts, it is hardly necessary to enter a protest against the ordinary view, that these craters are the result of cinder eruptions.* We remark only, that such eruptions will never take place except from small vents, for the cold which gives the viscidity on which they depend, necessarily contracts the area. In a large pool the fluidity is such, that the rising vapours pass off freely: the ejections over its surface, excepting those at the margin, will fall back again into the pool, as in boiling water and Kilauea, and could neither rise to the height, nor make such curves, as are represented by Mr Nasmyth. Instead of a large open crater having greater projectile force in proportion to its size, it will actually have far less; and within certain limits the force may be inversely as the diameter, though dependent also on the size of the chimney above.

Any vents in the moon in which the fires had partially subsided, would have densely viscid lavas from partial cooling; and in these there would be loftier ejections like those of ordinary volcanoes, forming high conical peaks with narrow openings, if any, at summit.

The great depth of the lunar pits seems to require another element for its explanation, in addition to what has been presented. This is supplied by the fact of the less specific gravity of substances on the moon; for objects on its surface have but one-sixth the specific gravity they have on the earth: that is, iron would weigh but one-sixth what it does here. The lavas would, therefore, not only be specifically lighter, but would become more blown up with the vapours, or more spongy. On the same ground, too, we understand why the moon's great craters should so generally terminate in a raised rim, while those of the earth, like Mount Loa, have very gently sloping sides and summit. This raised rim is fully illustrated about the Kilauea pools, as we have already stated: but in large overflowings, the earth's lavas, owing to their weight, flow far away by gravity, and this feature is, therefore, never exemplified on the earth on the same grand scale as in the moon.*

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* See the views and remarks of Mr J. Nasmyth, *loc. sit.*

We may, therefore, say unhesitatingly, without fearing an impeachment of our sobriety, that the moon's volcanoes are in fact volcanoes, either extinct or active, although the craters would receive comfortably more than a score of Etnas. We also comprehend the important fact, that a cooling globe would become at first a scene of great boiling lakes from the hardening of some portions;—that on a farther diminution of heat, these lakes would partially cool, excepting points or areas of greatest heat, and thus a subdivision of them would result; or else, they would *gradually* contract their overflowings, and so, as gradually, contract the size of the vent, obliterating all evidence of the former size: or again, they would more *abruptly contract*, and, consequently, form an inner ledge concentric with the outer walls, and perhaps also other concentric ledges still smaller.

This is well illustrated in the figures, and nothing could better indicate the mode of action which characterizes the moon's craters, for we may trace out the successive diminutions. In *Heinsius* (fig. 3), which is forty-eight miles in its longer diameter, this is beautifully shewn; there is one low ledge within another nearly concentric, and finally a smaller circular pit, of twelve miles breadth,—no mean size, though we call it small. An outer concentric ridge is also apparent through part of its circuit, which may have been a still earlier outline; and being lower than the ridge next within, it illustrates the statement, if the hypothesis be true, that the larger craters have lower walls. It is, however, possible that it may have resulted from a subsidence in the area around, as has happened at Kilauea. The same facts are still shewn by the mountains *Abulfeda* and *Timocharis;* and we have already remarked that the crater of the last mentioned has very nearly the features of the Kilauea pit. Again, they might finally so far diminish their size by the cooling in progress, that there would no longer be free ebullition, and the vapours having to force their way up, would break through with explosive violence, producing an alternation, it may be, of cinder and lava eruptions, or cinder eruptions alone at summit, and raising up conical pointed peaks. These different phases, in connection with fissure eruptions and upliftings from contraction, from both which causes ridges might result, give us a complete and comprehensive view of the origin of the moon's features.

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* The elevation theory of Von Buch has been supported from facts in the *moon. We offer* nothing here on that subject.

Are the lunar craters still active? To a very great extent the surface has evidently cooled; and whether there are any active points is a matter of doubt. But without admitting igneous action at the present time in some parts, how can the facts mentioned with regard to the difference in the light of different portions of the moon's surface (p. 17) be satisfactorily explained? This difference may possibly be partly accounted for on the ground (borne out by Kilauea), that the bottom of a crater may have a smoother surface than the declivities or plains exterior. Perhaps also there is something attributable to a difference of material, though this is not probable. If these explanations are received as sufficient for the craters, they fail of satisfying us with regard to the light streaks which are so remarkable about some cones—coursing over ridges and depressions without interruption. The fact of illuminated walls to a crater when the bottom is not illuminated, and the general diffusion of light when one or more bright areas of small extent may be distinguished, are also points not easily understood on the above suppositions. May it not be, that we should attribute some of the instances of lighted areas to a covering of vapours from the igneous action beneath? The light streaks are not depressions, and, therefore, not broad fissures having the great width they exhibit; but they may be *regions* containing many fissures from which vapours are escaping, and by the coalescence of such areas, the summit of a crater like *Euler* might appear illuminated. Such vapours might so cover the bottom of a crater that the walls would appear brightest. Moreover, they might leave the cones within a crater still distinct; for if spread out at a height of 500 feet above the bottom, they would still, in many instances, be more than 10,000 feet below the summit, and far below, too, the tops of interior peaks.

As there is little or no water in the moon to aid volcanic action, sulphur has probably played an important part in its igneous changes; for this is not only a prevalent means of igneous operations on our globe, but occurs in meteoric stones, pyrites being one of their common constituents. We may, therefore, believe that, wherever there is action in the moon, sulphur and any other vapourizable material present, are constantly escaping, either as simple vapour or in some gaseous combination, and forming a very low covering over certain portions.

As we have observed, the existence of actual volcanic eruption in the moon is still doubtful, and we must look to new facts to settle the point. But we cannot doubt that the surface in *former* periods has been everywhere in violent action, and that its pools of fire were once measured by scores of miles instead of by hundreds of yards, as with our existing volcanoes. And many of these immense basins remain still open for examination, presenting indications of the various changes which accompanied the gradual decrease of igneous action during the cooling in progress. A map of the moon, if there is any truth in these views, should be in every geological lecture room; for nowhere can we have a more complete or more magnificent illustration of volcanic operations. Our own sublimest volcanoes would rank among the smaller lunar eminences; and our Etnas as but spitting furnaces.

In continuation, I would ask attention to some thoughts bearing on our own planet, which are suggested by this study of the moon's surface.

If the earth was once a melted globe, it must have passed through the same phases as the moon, with this *very important difference*, that the whole surface during its progress was subject to the denuding action of waters, and from the first had valleys and sedimentary rocks in progress. It must have had originally its boiling pools of vast extent; which, as the action decreased in violence would more or less gradually contract. Are there any remains of these great craters? Or have they disappeared by a decrease in the volcanic action and thus graduated into existing mountains, or have they been swept away by the changes of time? M. Von Buch has described a circular area on the island of Palma, one of the Canaries,* six miles in diameter, which has been compared to a lunar crater, with some appearance of reason. It is, in fact, hardly twice the diameter of Kilauea, which it otherwise resembles. On Mauritius there is a similar area, fifteen miles in diameter, surrounded by precipitous walls composed of the edges of strata dipping outward.† Either it is a volcanic mountain whose centre has fallen in, as suggested by M. Bailly, or it is the remains of a great pit-crater. I merely state the fact without expressing an opinion. Other instances might be mentioned, but this will suffice. At the present period, few active boiling pits remain, and Kilauea is the only one whose characters have been well determined. The surface fires of the globe have so far subsided in action, that in nearly every existing volcano, cinder ejections characterize the action at summit, and eruptions of lavas in streams are confined to fissures through the sides and flanks of the mountain.

II. We are led by the facts displayed, to remark also on the *origin of the mineral constitution of igneous rocks.*

It has been a difficult problem for solution, why volcanic regions should have a centre of solid feldspathic rocks, unstratified and compact, while the exterior consisted mainly of basaltic lavas. Scrope, Von Buch, and other writers on volcanoes, have mentioned instances of this structure; and it seems to characterize generally the large volcanic mountains. It is well exhibited when the elevations are cut through by gorges; and when not, the clinkstone appears often at the summit of the cone or dome. The explanations we here venture, proceed on two principles:

1. *The motion which belongs to a boiling fluid.*
2. *The less fusibility of feldspar than the other ingredients.*

In the great boiling pools, there will necessarily be a rising of the fluid, in the hotter part, and a flow away towards either side, producing a kind of circulation. This is no hypothesis, as the fact may be witnessed in any boiling cauldron; and the lavas of Kilauea are a visible example of it. The ebullition in lavas on the earth, proceeds principally from the vapours of water and sulphur, which are constantly rising through them, inflating them more and more as they ascend, and finally escaping in bubbles at the surface. Now the feldspar being the less fusible part of the lavas, would thicken somewhat, wherever the temperature became too low for complete fusion; the more liquid portion would then ascend most easily, being carried along by the inflating vapours, and much of the feldspar would thus be left behind, and it might be in a nearly pure state. The centre of the volcano under this action becomes necessarily feldspathic. The summit might, therefore, eject either basaltic or feldspathic rocks from the material of the vent; though when the action was violent and deep, it would eject feldspathic rocks alone.

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* Desc. Phys. des Iles Canaries. Paris, 1836, p. 281.

† Darwin's *Volcanic Islands.* London, 1844, p. 30.

At the same time the *basaltic* lavas, descending laterally in this system of circulation along the sides of the great central conduit, may pass out as flank eruptions through fissures. Besides, there will also be basaltic ejections from sources of lavas at a distance from the central conduit, where they have not been subjected to the separating process described; and this may be the more common source.

Mountains with a feldspathic centre, and basaltic layers forming the circumference, are therefore quite intelligible without supposing the feldspar to have been first thrown up, or appealing to a different system of fissures for their origin, and the examples which the moon presents, are more extensive than is necessary to explain the widest facts on the earth.*

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* It is common to say that certain domes of trachyte were thrown up in a *pasty* or imperfectly fluid state, in order to account for the fact, that there is no appearance of the rock's having flowed in streams. Without intending to refer the origin of these domes to any particular cause, I would suggest the query, whether, if their formation was subaërial, this *pasty* state does not necessarily imply that the ascending vapours would have found some difficulty in escaping, and would have broken through with explosions, as explained in the foregoing pages; and, consequently, that there would have been scoria and cinders accompanying the ejections? or may we believe it probable, that the paste was so dense that water would not make its way up and escape as vapour? Is

In these remarks we have spoken of the lavas as consisting mainly of feldspar and augite, their more common constitution; but we use the terms in a general sense, understanding by feldspar one or another of the feldspar family of minerals, and by augite the remaining fusible material, whether ordinary augite (silicate of lime, magnesia and iron), or silicates of one or more of these bases or alumina in other combinations.

There is some difficulty in applying this hypothesis to particular cases, on account of our ignorance of the actual fusibilities of the materials of the lava, in the condition in which they are placed; for we know that an infusible mineral may be held in fusion under certain circumstances, or with certain mineral associations, far below the temperature at which it fuses; or, previous to the commencement of cooling, it may be in some other combination.

We should infer that the process which separates the feldspar, would also separate any excess of the more infusible mineral quartz. This may not follow: still it is a remarkable fact, that the quantity of quartz contained in trachyte is often in great excess, as analyses have shewn. But why is not the infusible mineral chrysolite also detained? The fact appears to be, that it is of subsequent formation. The small proportion of silica it contains implies a deficiency of this substance; while, as we have stated, in the feldspathic rocks there is often an excess. It may, therefore, under certain circumstances proceed from the basaltic material, for its elements are the same in different proportions. Subsequent investigations may give us more light on this point.

The general principle which we have above brought forward, is well illustrated in the fact, that the scoria or surface glass of any vent, where it occurs, is the most fusible part of the lava, consisting, in general, of ferruginous or alkaline silicates, and containing no magnesia. On account of the diminished heat, this material alone remains sufficiently fluid to be inflated and borne up to the surface by the rising vapours: and this takes place in spite of superior gravity.

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this last supposition borne out by any existing example of subaërial volcanic action?

We hence comprehend the *rapid cooling* which characterizes ejected lavas, *for only a part of the material is in complete fusion.*

The actual nature of the *cooled igneous rock* may be more correctly understood, if we consider that the minerals present will depend, not only on heat and pressure, and the causes above alluded to, but also on rate of cooling. The effect of slow cooling is exemplified in the feldspathic centre of a volcanic mountain. Being wholly enclosed by rocks, the heat of fusion passes off slowly, and owing to the pressure of its own superincumbent portions, the rock is compact. Whatever augite may be present, instead of appearing as augite, will take the form of hornblende, a mineral which requires slow cooling, and differs from augite in the crystalline form which it thus receives. In corroboration of this statement, hornblende is common in trachÿtes and such feldspathic rocks. The same remarks apply to mica: and other minerals also may form according to the elements present. Chrysolite is not met with: it occurs only where there is a more rapid rate of cooling, as in the formation of ordinary basaltic rocks or lavas.

Farther, we observe, that with a still more gradual rate of cooling, the whole feldspathic rock becomes crystalline in texture like a granite or syenite, and it is well known that granite-like or syenitic rocks or peaks occur in some volcanic regions, whose interior has been laid open by denudation. Many minerals, too, might crystallize under these circumstances, which, with more rapid cooling, would not be distinguishable.

The boiling process in a large volcano, therefore, in connection with the circumstances of temperature, rate of cooling, the fusibilities of different minerals, and the other causes alluded to, will account for the various features, positions, and relations of igneous rocks, and for many facts relating to the distribution of igneous minerals.* We may hence reasonably infer that granite and granite minerals may form under the same circumstances, if the elements are present in the material in fusion; for the syenites alluded to are closely allied rocks in texture and character. At a former meeting of this Association, it was suggested by me that *some* regions of granite peaks may have been centres of ancient igneous action; and their being surrounded or bordered by hornblendic rocks, seems to point to some actual analogy with the trachytic centres and basaltic circumference of mountains admitted to have been volcanic.

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* The general causes referred to, act under the guiding laws of crystallogeny, which laws regulate the particular positions of minerals according to the principles exemplified in segregations or radiated crystallizations, and the laminated or cleavable structure of igneous rocks.

The opinion that the nature of the resulting rock is directly connected with the nature of the rock which had entered into fusion, cannot be maintained if the above views be true. On the contrary, it appears that while the result may thus be varied, the mode of distributing minerals in a volcanic focus by the boiling process, may produce, from the same material, rocks of a predominant feldspathic character in one place, and rocks of a hornblendic or augitic character in other places. Simple feldspathic granites may be fused and ejected as feldspathic rocks, like those of porphyry dikes. But it is an interesting fact, that the rock of most dikes is of the augitic (or hornblendic) kind, like the dikes of volcanoes that rise from sources in which the *separating* process could not have been operating.*

We also arrive at the important conclusion, that rocks perfectly compact in texture may be of subaërial origin, as we have pressure from the fluid lavas themselves in the volcanic focus.

Another deduction proceeds from the facts stated;—that the same igneous rocks may occur of all ages, provided the atmosphere or waters of the earth were not too warm for the more rapid rate of cooling required for uncrystalline rocks. Scoriæ, basalt, trap, porphyry, syenite, granite, have no relations to one epoch rather than another, beyond what may depend on the circumstance just mentioned. Whenever, therefore, in the history of the world, the variations in heat, pressure, and rate of cooling, now possible, may have taken place, similar rocks to those of the present day may have been in progress:—and as far as the variations of former times, in these respects, may now take place, former rocks and minerals may still be in progress. In this statement it is implied that the necessary elements are present in the fused material.

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* Mr Darwin has accounted for the distribution of feldspathic and augitic rocks in volcanoes, on the ground of their different specific gravity. But with this cause alone, the lower parts of the feldspathic peaks should be expected to contain the heavier augitic material, which is not the case. He also argues that the feldspar would rise in the fluid as crystals, and so the augite sink. But we know, in the first place, that crystals do not appear till incipient solidification; and, if the augite and feldspar were both distinct crystals, where would be the fusion? Again, the feldspar rocks are *amorphous*, except with a very slow rate of cooling; and how, then, can the existence of appreciable crystals be assumed?

III. *Origin of Continents.*—The moon gives us hints on another topic of great interest, relating to the distribution of land and water on our globe. We have mentioned that there is a large area covering nearly one-third of the hemisphere facing the earth, which is mostly free from volcanoes, while on other parts the craters are closely crowded together. We may therefore reasonably infer, that over this naked portion the surface first became solid, and has therefore cooled the longest and to the greatest depth. Consequently, the contraction from cooling, which was going on, would take place most rapidly over the thinner and more yielding volcanic portion; and unless the ejections made up the difference, this part would become somewhat depressed. A melted globe of lead or iron in the same manner, when cooling unequally, becomes depressed by contraction on the side which cools last. Now on our own globe, the continents have, to a very great extent, been long free from volcanic action. A glance at a map of Asia and America will make this apparent. It is usual to attribute this almost total absence of volcanoes from the interior of the continents to the absence of the sea; but it is fatal to this popular hypothesis, that the same freedom from volcanoes existed in the Silurian period, when these very continents were mostly under salt water, a fact to which the wide-spread Silurian rocks of America and Russia testify. Over the *oceans*, on the contrary, all the islands except the coral are igneous; and the coral may rest, as we have reason to believe, on an igneous base.

It is, therefore, a just conclusion, that the areas of the surface constituting the continents were first free from eruptive fires. These portions cooled first, and consequently the contraction in progress affected most the other parts. The great depressions occupied by the oceans thus began; and for a long period afterward, continued deepening by slow, though it may have been unequal, progress. This may be deemed a mere hypothesis; if so, it is not as groundless as the common assumption, that the oceans may have once been dry land,—a view often the basis of geological reasoning.

Let us look farther at the facts. Before the depression of the oceanic part of our globe had made much progress, the depth would be too shallow to contain the seas, and consequently the whole land would be under water. Is it not a fact that, in the early Silurian epoch, nearly every part of the globe was beneath the ocean? So we are taught by the extent of the formation. The depth of water over the continental portions would be very various; but those parts which now abound in the relics of marine life, were probably comparatively shallow, as amount of pressure, light, and dissolved air, are the principal circumstances influencing the distribution of animals in depth, and acted formerly, we may believe, as at the present period. Here, then, we see reason for what has been considered a most improbable supposition, the existence of an immense area covered in most parts by shallow seas, and so fitted for marine life.

If we follow the progress of the land, we find that with each great epoch there has been a retiring of the sea. In the coal deposits we have an abundant land vegetation. Subsequently, the progress on the whole was giving increased extent and height to the land, and diminishing the area of the waters. Instead, therefore, of a bodily lifting of the continents to produce the apparent elevation, it may actually have been a retreating of the waters through the sinking of the ocean's bottom. The process, however, has not been a continuous one; for during each epoch—the Silurian and the more recent—there have been subsidences as well as seeming and actual elevations, and various oscillations of the continental surface, from subaërial to submarine, and the reverse. When contraction had once taken place over the continents, as well as under the ocean, there may have afterwards been expansions again through the return of heat from some cause. And thus various irregularities have taken place, such as the rocks indicate. In the tertiary period, and since, the apparent rise of the land has been still to some extent in progress. And is there any evidence that this could have arisen from a sinking of the ocean's bed? The evidence is undoubted. For Mr Darwin has shewn satisfactorily (and farther observations to the same end, and to many interesting conclusions, will be presented in the writer's Geological Report on the Pacific), that a subsidence of some thousands of feet has taken place since the corals commenced their growth. Every coral island is a register of this subsidence.*

And why should not the ocean's bottom subside, as well as the land? What has given the continental portions of our globe their elevation, as compared with other parts, if not the unequal contraction of the whole? Can we safely affirm —in words of high authority—" that the stability of the sea and the mobility of the land are demonstrated truths in geology,"† when mobile land forms also the bed of the ocean, and its changes must affect the stability of the superincumbent waters;—I ask, can we safely make this affirmation, until we know something more certain than past investigations have revealed, about the geological history of the two-thirds of the surface of our planet that are concealed beneath its oceans?

In our conclusions from the above reasoning, we fall in nearly with the views presented by a distinguished French geologist, M. C. Prevost, who has argued with much force in favour of subsidence as a cause of the apparent elevation of the land; though it may be right to state, that these conclusions were arrived at previously to seeing his memoir.* There

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* See Silliman's Journal, xlv., p. 131, 1843.

† Leonard Horner, Esq., Anniversary Address before the Geological Society of London, January, 1846; Quarterly Jour. of the Geol. Soc., No. 6, p. 199.

* The general theory of changes of level by contraction and expansion, and

ear to be many objections to the opinions of M. Prevost,
they are expressed by him, inasmuch as no allowance is
le or admitted for minor disturbances and actual eleva-
s by subterranean forces. His views, however, are well
thy the attention of the geological inquirer.

The principles explained place the general theory of change
level by contraction upon something better than a hypo-
tical basis, and are believed to explain the actual causes
which the changes have been produced. They correspond,
reover, with the view that ruptures, elevations, foldings,
d contortions of strata have been produced in the course
contraction. The greater subsidence of the oceanic parts
uld necessarily occasion that lateral pressure required for
e rise and various foldings of the Alleganies and like re-
ions.

A brief Review of the Classification of the Sedimentary Rocks of Cornwall. By Sir RODERICK IMPEY MURCHISON, G.C. St S., F.R.S., V.P.G.S. & R. Geogr. S., &c., &c. (In a Letter, addressed Sir C. LEMON, Bt., M.P.) Communicated for the Journal by the Author.

MY DEAR SIR CHARLES,—In compliance with the promise I made when attending your last anniversary meeting at Penzance, I now

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the rise thus of continents, was first presented by Mr Babbage and De la Beche. M. C. Prevost takes the different ground, that all seeming elevations are the result of subsidence. His propositions are as follows (Bulletin de la Soc. Geol. de France, xi., 1839 à 1840, p. 186):—

"1. Que le relief de la surface du sol est le résultat de grands affaissements successifs, qui, par contre-coup, et d'une manière secondaire, ont pu occasioner accidentellement des élévations absolues, des pressions latérales, des ploiements, des plissements, des ruptures, des tassements, des failes, etc.; mais que rien n'autorise à croire que ces divers accidents ont été produits par une cause agissant *sous le sol*, c'est-à-dire par une force soulevante;

"2. Que les dislocations du sol sont des effets complexes de retrait, de contraction, de plissement et de chute;"

"3. Que les matières ignées (granites, porphyres, trachytes, basaltes, lavas), loin d'avoir soulevé et rompu le sol pour s'echapper, ont seulement profité des solutions de continuité qui leur ont été offertes par le retrait et les ruptures, pour sortir, *suinter et s'épancher au-dehors*."

give you a more decided opinion than I was then enabled to do, respecting the age of the lowest and oldest of the sedimentary rocks of Cornwall.

Not having seen the fossils collected by Mr Peach on the south coast of Cornwall, I then found it difficult to come to any other conclusion than that at which Professor Sedgwick and myself had long ago arrived; viz., that, with the exception of the presence, in the north-eastern extremity of the country, of a portion of the culmiferous (carboniferous) trough of central Devon, the remaining and underlying strata of Cornwall were of the age of the Devonian or old Red system. The few Cornish fossils which were then shewn to me in your museum, were unquestionably similar to those with which I was formerly familiar in Devonshire and North Cornwall, as well as with those of the Rhenish provinces and the Eifel, which Professor Sedgwick and myself had shewn to occupy a like geological position. They were in fact, forms of the same type as those which, at the suggestion of Mr Lonsdale, and with the assistance of Mr James Sowerby on one occasion,* and with the help of MM. de Verneuil and d'Archiac on another,† we had published as characteristic of a group of intermediate characters, pertaining to strata lying *beneath* the carboniferous rocks and *above* the Silurian system. In a word, they were identical with some of the numerous fossils of Devon and North Cornwall, published in the work of Professor Phillips;‡ who, in pointing out in certain tracts the connection of this group with the carboniferous fossils, which he had so well described, and in others with the Silurian forms I had published, had also concluded that the great mass of fossiliferous strata which rise up from beneath the culm measures of central Devon were of the same intermediate characters. In his valuable maps of Cornwall and Devon, Sir H. de la Beche gave essentially the same views of geological succession; and, lastly, in his report upon the geological structure of that region, he described certain detailed sections in the southern districts of Cornwall, to which I will presently advert.

In proposing the word "Devonian," as applied to the intermediate strata in question, Professor Sedgwick and myself thus qualified our meaning in regard to the extension of such rocks into Cornwall:—"In asserting that the stratified rocks of Devonshire and Cornwall are, upon a broad scale, the equivalents of the carboniferous and old red systems, we do not however deny, that, in certain tracts, *the lowest members of some of these rocks may represent the upper division of the Silurian system;* for although we have as yet found few if any of the fossils most typical of that system, we admit that when the sediments of a given epoch have been accumulated under peculiar conditions, we must expect to find considerable variations in the forms of animal life. Again, we know that the rocks of this region have undergone great changes in assuming their hard and slaty character; and under such circumstances, the difficulty of precisely limiting the boundary line of any portion of them is prodigiously increased."*

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* Trans. Geol. Soc., N.S., vol. v., p. 633.

† Idem, vol. vi., pp. 221, 303.

‡ Palæozoic Fossils of Devon and Cornwall.

The truth is, that neither Sir H. De la Beche and Professor Phillips, nor Professor Sedgwick and myself, had, at the time when our works were published, seen any fossils from *South* Cornwall sufficiently distinct to warrant the conclusion, that it contained forms of an older type than those which had been detected in North and South Devon, and in the west of Cornwall. It was, therefore, believed (and all geological maps were coloured accordingly) that the zone of rocks occupying the southern headlands of Cornwall, between the Bay of Plymouth on the east, and the Lizard Head on the west, were simply downward expansions of the fossiliferous "Devonian" strata. In this state of the question, your associate Mr Peach began his labours in collecting fossils along the southern headlands of Cornwall. He first ascertained that certain forms first discovered by Messrs Couch in the environs of Polperro were fishes, which he exhibited at the Cork Meeting of the British Association, and concerning which Professor Phillips and myself could only venture (so obscure did they appear to us) to give the guarded, though suggestive opinion, which Mr Peach has recorded in your thirtieth Report. I then ventured to surmise, that these ichthyolites might belong to the Upper Silurian rocks, the oldest in which the remains of any vertebrated animals had yet been discovered, because "they occurred in rocks forming the axis of South Devon and Cornwall, which I had always considered to be the oldest in that country."

In pursuing his researches, Mr Peach published, in 1844, a synopsis of the Cornish fossils from various localities, in which, besides the ichthyolites of Polperro, he identified several mollusca from Gorran Haven, Caerhayes, and Carn Gorran Bay, with typical Silurian species. These were the fossils I was so anxious to see at Penzance; and Mr Peach having obligingly forwarded them to me in London, I no sooner unpacked the box, than I found that true Silurian and even Lower Silurian rocks existed in Cornwall,—the proofs being the presence of certain simple-plaited *Orthidæ*, which are invariably typical of that age. But although Mr Peach had come to a correct general conclusion, the specific names he attached to the South Cornish fossils in your thirtieth Report are not correct. In respect to the ichthyolites from the slates of Polperro, Pentuan, &c., they have been referred to our mutual friend Sir Philip Egerton, who his better versed in the classification of Agassiz than any of our countrymen, and he thus writes to me concerning them:—" These remains are very enigmatical, and I cannot identify a single specimen with any form I know. I do not think any one of the fragments belongs either to *Cephalaspis* or *Holoptychius*. The nearest approach is to *Bothriolepis*. The dorsal fin, named by Mr Peach, *Onchus Murchisoni* (Agass.) is not so, as far as I can determine from the description of Agassiz, unless it be a more perfect specimen than he has seen. The longitudinal ribs, instead of being uniform (as figured by Agassiz), are notched, more after the manner of *Ctenacanthus*. The other *Onchus* may be *O. tenuiserratus*, but I have not here the means of comparison. From the general appearance of the collection, I should say they differ from any old Red or Devonian fishes I have ever seen."

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* *Phil. Mag., 1839, vol. xiv., p. 241.*

If these ichthyolites do not decisively help us to settle the age of the Polperro zone of rocks, they are still of great interest, as being the only group of fishes worth noticing which has been found in the older rocks of Devonshire and Cornwall,* and, also, as being associated with shells which Mr J. Sowerby indentifies with the *Bellerophon trilobatus* (Sil. Syst.) and the *Loxomena lincta* (Phillips). The first-mentioned of these shells is characteristic of the tile-stones in Herefordshire and Shropshire, and is also found in strata of the same age in Cumberland (between Kirby Lonsdale and Kendal), which form the uppermost band of the Silurian rocks, or, a transition from Silurian into the Devonian system. Now, as Professor Sedgwick and myself had inferred that the limestones of Looe and Fowey belonged to the lower calcareous zone of Devonshire, and as the sections of Sir H. De la Beche shew that the Polperro beds dip beneath the Looe and Fowey rocks, the zoological evidences seem to harmonize with recorded physical facts, and we thus obtain reasonable grounds for believing, that the lowest Devonian, if not the uppermost Silurian, strata, are exposed in the district which ranges along the shores of that part of Cornwall, by Polperro, Pentaun, &c.

But, if doubts should exist as to whether the Polperro slates ought to be referred to the bottom of the Devonian or top of the Silurian system, the discoveries of Mr Peach in the headland of the Dodman, and in the prolongation of its strata to Veryan Bay, completely demonstrate, that still older and unquestionable Silurian rocks are there present. This is the district in which both Professor Sedgwick and Sir H. De la Beche had noted the existence of a line of elevation,† running from NE. to SW., which, bringing up certain quartzose or argillaceous slates, had thrown the beds off, both to the NE. and SW., the published section of the latter having clearly indicated these relations.

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* Professor Phillips mentions two very imperfect and doubtful scales of fishes, the one in South Devon, the other in North Devon. Palæ. Fossils, p. 133, figs. 256, 257.

† See Trans. Geol. Soc., N.S., vol. v., p. 666; and Report on Cornwall and Devon, p. 84.

The fosssls found by Mr Peach at Gerrans Bay, as determined by Mr J. Sowerby, are *Orthis lata*, *O. orbicularis*, another species resembling *O. plicata*, and a fourth, which does not appear to have been published. At Caerhayes, Mr Peach has collected other forms of *Orthidæ*, one of which approaches nearest to the *O. alternata* of the Silurian system. The remainder are not, however, referrible, as he had supposed, to *Leptæna lata*, *Terebratula nucula*, *Atrypa striatula*, &c. The fossils from the Great Peraver quarries in Gorran Haven, on the eastern face of the Dodman, are still more decisive; for the species which Mr Peach has named *Orthis flabellulum* and *O. testudinaria* both belong, unquestionably, to the *Orthis* (*callactis* B)[*] *calligramma* (Sil. Syst.), and with it is a form undistinguishable from the *Orthis* (*canalis*) *elegantula* (Sil. Syst.). The only well preserved *trilobite* in this rock appears to me to be the *Calymene pulchella* (Dalman); a second species resembles *C. Blumenbachii*.

No one accustomed to the Palæozoic rocks can throw his eye over the fossils from these three localities, without at once recognizing them as true Silurian types. They have an entirely distinct *facies* from the fossils of the overlying Devonian system; none of the species so abundant in North-western Cornwall are here present. With my imperfect knowledge of the country, it would be premature to say, that subdivisions can be established in this highly dislocated region, so as to define Upper and Lower Silurian bands. But it may safely be asserted, that the fossils of Gorran Haven are Lower Silurian types; there being no one species more characteristic of the inferior portion of that system than the *Orthis caligramma*, which, in Shropshire and the adjacent Welsh counties, is found to range downwards, from the very uppermost beds of the Caradoc sandstone into the heart of the Snowdon slates, and is equally typical of the Lower Silurian rocks of Russia and Scandinavia. At the same time, I do not think that the Gorran Haven beds lie deep in the Lower Silurian group: they probably represent the upper portion only of the Caradoc sandstone; for the *Orthis canalis*, or *elegantula*, and the *Calymene pulchella*[†] (Dalm.), closely allied to *C. Blumenbachii* are Wenlock, as well as Caradoc, fossils. Judging from the fossils only, I should say that the beds at Gerrans Bay with the *Orthis orbicularis* are younger than those of Peraver and Gorran Haven. Time and careful researches must, however, determine this question of detail, and all I can now express is my opinion, that the quartzose rocks and killas which extend from the tracts above alluded to, to the mouth and centre of the bay and harbour of Falmouth (probably much farther to the SW.), are of Silurian age also.

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[*] This shell was called *Orthis callactis* B in the Sil. Syst., Plate 19, fig. 5, but subsequent comparisons have shewn that it is identical with the *O. calligramma* (Dalman) of Scandinavia, Russia, &c.

In like manner, the *Orthis canalis* of the Silurian System has proved to be the *O. elegantula* of Dalman; and the names of that author being the oldest, are now necessarily adopted.

[†] The *Calymene pulchella* (Dalm.) occurs both in the inferior part of the Upper Silurian, and the higher part of the Lower Silurian, in Sweden, Siluria, and Wales.

The energy of Mr Peach having thus afforded us the key by which new lights are thrown on the succession of Cornish strata, I cannot but hope, that when the Government geological surveyors revisit Cornwall, they will define the exact demarcations between these Silurian masses and their overlying Devonian neighbours. In fact, I have, within these few days, been talking over this subject with my friend the Director-General of the survey, and he has pointed out to me, on his detailed map, how, from the enormous flexures which the strata have undergone in their range from Devon into Cornwall, it is highly probable that Silurian rocks (the equivalents of those alluded to) may be recognised in other parts of Cornwall. Thus, the quartzose rocks of Pydar Down or Moor, to the north of St Columb, which form an E. and W. axis, dipping to the N. and S. under fossiliferous Devonian strata, may (he thinks) prove to be also of Silurian age. But, forbearing to speculate on the probable results of future researches, it is my decided belief, that the slaty rocks, constituting the great southern headland of Devonshire; at least all the schists, &c., to the north of the Start Point, will eventually be classed with the Silurian group of South Cornwall: for if the Plymouth group of limestones, so prolific in animal remains, afforded us the means of deciphering the age of less clearly developed zones on the same horizon in Cornwall, the Silurian types collected by Mr Peach may enable us to carry out a more correct classification in still older strata, from Cornwall into the obscure southernmost promontory of Devonshire. In the mean time, confining ourselves to what we now know, it is manifest that Cornwall exhibits in ascending order,—*1st*, a band of true Silurian rocks; *2d*, a zone of intermediate character, forming a transition between the Silurian and Devonian systems; *3d*, a copious Devonian system, characterized by lower and upper limestones; and, *4th*, a limb of the culmiferous or carboniferous basin.

This view will, I trust, be perfectly intelligible to the members of your Society who have occupied themselves with the consideration of this branch of geology, and on which Mr R. Q. Couch has recently written with perspicuity and talent. I doubt, however, if anything I have stated will make an impression upon one of that number, my good-humoured antagonist the Rev. D. Williams, whose views of the Cornish succession of strata seem to be opposed to those of all his contemporaries. Geologists, however, who have long lived in Cornwall, and have so well illustrated its mineral structure, will, I am persuaded, be the first to admit the value of the Palæozoic classification, which, having been worked out and established in tracts exempt from much dislocation and alteration, has been so applied as to enable us to interpret the true history of the highly convulsed and metamorphosed rocks of their county. It is, in fact, the greatest triumph which could have been anticipated on the part of those who have steadily proceeded from the known to the unknown.

Looking from your own country to the opposite side of the Channel, you are doubtless well aware that there is the strongest analogy between the slates and granites of Cornwall and those of Brittany and Normandy. Many persons have remarked upon the strong resemblance between the Mounts St Michael in the two countries, and no one can have traversed these two regions without perceiving, that just as they evidently belong to the same mineral type, so are their respective inhabitants descended from a common stock, whose names of places have passed down to their French and English descendants. The existence of Caradoc sandstones and other Silurian rocks in that part of France, having been already indicated (Devonian and carboniferous strata being also abundantly developed), their discovery in Cornwall is a happy addition to that union of geological and historical records, by which these widely separated residences of the Celtic race are illustrated.

With regard to the highly mineralized or metamorphosed conditions of great portions of the killas and sandstone of Cornwall, I can do little more than refer you to the few observations I made at your anniversary meeting, and of which a brief abstract has appeared in your newspapers. There are, as you well said in your Anniversary Discourse, many analogies between the metamorphic rocks of Cornwall on the one hand, and those of Scandinavia and the Ural Mountains on the other; whilst the parallel is now drawn closer by the recent discovery of the Cornish Silurian rocks. Your last erupted granites, elvans, and porphyries, have played exactly the same part, in traversing your Palæozoic sediments, as like rocks have done in Norway. Cornwall may also be compared to large portions of Siberia, and notably to the Ural Mountains, whose chief eruptions have taken place through deposits of Silurian, Devonian, and carboniferous age. In both countries, the eruptive rocks are granites, porphyries, greenstones, and serpentines: even in their superficial accumulations, there is this striking resemblance, that the Cornish detritus and gravel (as clearly pointed out by Mr Carne) is purely *local*;—the county being quite as exempt from all far transported materials as the Ural Mountains and Siberia. With this absence of all foreign transport or drift, Cornwall is as instructive as the Ural in never exhibiting those "roches moutonnées," and those polished and striated surfaces, which have been so erroneously referred to the action of land glaciers, in all those low regions of the earth where they have clearly been caused by the action of powerful aqueous drift, in the manner I have elsewhere attempted to explain.*

Your *stanniferous* gravel bears, indeed, precisely the same relations to your granites and killas, as the *auriferous* deposits of the Ural to the eruptive and schistose rocks of that chain. Both are mere local, shingle accumulations, derived from veinstones which have been denuded from the surface of adjacent crystalline rocks. With these analogies there is, however, a marked distinction between Siberia and Cornwall. All richly auriferous chains (Humboldt first remarked the fact) have a meridian direction, as in the Ural, and various N. and S. parallel ridges in Siberia and other parts of the globe. The axis of Cornwall, on the contrary, is transverse to that direction, viz., from ENE. to WSW.; and though containing copper ore in common with the Russian mountains, it differs from them in not producing gold or platinum; whilst it is peculiarly distinguished by containing tin, which is unknown in the Ural. Let us hope that the day is fast approaching, when the cause of the production of such striking phenomena as these will receive some explanation at the hands of those physical philosophers, who are advancing a line of research in which your own countryman, Mr R. W. Fox, has already so distinguished himself. But if gold exist not (in any appreciable quantity at least) in your otherwise richly endowed mineral county, there are, I am happy to say, good grounds for hope, that in their most distant colony Englishmen may find it abundantly. In an address to the Royal Geographical Society, delivered in May 1845, when commenting upon the valuable labours of Count Strzelecki in deciphering the structure of the great N. and S. chain which ranges along the eastern shores of Australia, I specially insisted upon its striking resemblances to the Ural Mountains, whether in direction, in structure, or in alluvia; remarking, by the way, that *as yet* no gold has been found in this alluvium. I now learn, however, that fine specimens of gold have been found on the western flank of the Australian cordillera, particularly at the settlement of Bathurst, where it occurs in fragments composed of the same matrix (viz., quartz rock) as in the Ural. My friend and associate in the Imperial Academy of Petersburg, Colonel Helmersen, has recently suggested, that a careful search for gold ore in the Australian detritus will, it is highly probable, lead to its detection in abundance; since the Russians had long colonized the Ural Mountains, and had for many years worked mines of magnetic iron and copper in solid rocks, before the neglected shingle, gravel, and sand, on the slopes of their hills and in their valleys, were found to be auriferous. If, then, in the course of your statistical inquiries, you may know of any good Cornish miner about to seek his fortune in Australia, be pleased to tell him to apply his knowledge of the mode of extracting tin ore from his own gravel to the drift and debris on the flanks of the great north and south chains of Australia, or any smaller parallel ridges of that great country;* for great would be my pleasure to learn, that through the application of Cornish skill, such a region should be converted into a British "El Dorado."

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* See Russia in Europe and Ural Mountains; and Journal of the Geol. Soc., *No. 8.*

Requesting you to pardon this little digression, which, after all, may be turned to profit, and hoping that you will be as proud as I am of the connection which is now established between Cornwall and Siluria, believe me to be, my dear Sir Charles, yours most faithfully,

ROD. I. MURCHISON.

Geographical Distribution of Zoophytes. By JAMES D. DANA.

Heat, light, pressure, and means of subsistence, influence more or less the distribution of all animals; and to these causes should be added, for water species, the nature or condition of the water, whether fresh or marine, pure or impure, still or agitated. Next to the character of the water, heat is the most prominent limiting agent for marine animals, especially as regards latitudinal extent; while light and hydraulic pressure have much influence in determining their limits in depth.

Although these causes fix bounds to species and families, they do not necessarily confine tribes of species to as small limits. This is sometimes the case, and it is nearly true of a large group of zoophytes; yet other tribes and orders include species whose united range comprises all the zones, from the equator to the polar ices, and every depth, to the lowest which man has explored, affording traces of life.

Order Hydroidea.—The Hydroidea are met with in all seas, and at great depths, as well as at the surface. The tropics, and the cold water of the frigid zone, have their peculiar species, and a few are found in fresh waters. The rocks and common marine plants of the sea-coast, the dead or living shell, or the floating fucus of the ocean, are often covered with these feathery corals; and about reefs they occasionally implant themselves upon the dead zoophyte, forming a mossy covering, taking the place of the faded coral blossom.

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* It appears that gold ore has been also found in the metalliferous ridges of *South-western Australia (Adelaide, &c.).*

The species are most abundant, however, in the waters o the temperate zone, and are common upon some portions of our own coast.

Order Actinoidea.—The Actinoidea are marine zoophytes. All oceans have their species, yet in the torrid zone they more especially abound, and display most variedly their colours and singular forms.

The soft Actinidæ and the Alcyonaria have the widest range, occurring both among the coral reefs of the equatorial regions, and to the north and south beyond the temperate zone. The Mediterranean affords species of Gorgonia, Corallium, and Alcyonium, besides numerous Actinoidea. The coasts of Britain have also their Alcyonia and Actinia; and from far in the northern seas come the Umbellularia, and some other species of the Pennatula family.

Among the coral-making Actiniaria, the Madrepore and Astræa tribes are almost exclusively confined to the coral-reef seas, a region included mostly between the parallels of 28° north and south of the equator, while the Caryophyllia family are spread as widely as the species of Actinia. Several species of Caryophyllidæ occur in the Mediterranean, and others in the high northern seas, and they are met with at depths of several hundred feet. They are also common among the coral reefs of the tropics.

The Madreporacea and Astræacea, with the Gemmiporidæ, are the principal constituents of coral reefs. The temperature limiting their geographical range is 66° or 68° F., this being the winter temperature of the ocean on the outskirts of the reef-growing seas. The waters may sometimes sink to 64°, but this appears to be a temperature which they can endure, and not that in which they germinate. The extremes which they will survive prove only their powers of endurance, and do not affect the above statement; for their geographical distribution will be determined by the temperature which limits their powers of germination.

The temperature of the ocean in the warmest parts of the Pacific varies from 80° to 85°, and here Astræas, Meandrinas, Madrepores, &c., are developed with peculiar luxuriance, along with thousands of other strange and beautiful forms of tropical life. A range from the above temperature to 72° does not appear to be too great for the most fastidious species. At the Sandwich Islands, which are near the northern limits of the coral seas, Porites and Pocilloporæ prevail, and there are very few species of the genera Astræ, Mussa,* and Meandrina, which are common nearer the equator.

The range of these reef-forming corals in depth is singularly small. Twenty, or perhaps *sixteen*, fathoms will include very nearly all the species of the Madrepore and Astræa tribes.† Temperature has little or no influence in occasioning this limit, as 68° F. will not be found under the equator short of a depth of 100 fathoms. Light and pressure—the latter affecting the amount of air for aëration, are probably the principal causes. The waves, moreover, cannot aid in renewing the expended air below, as they do at the surface.

In recapitulation, we state, that the Astræacea, Madreporacea and the Gemmiporidæ among the Caryophyllacea, are, with few exceptions, confined to the coral-reef seas,‡ and to within 20 fathoms of the surface. The Caryophyllidæ§ extend from the equator to the frigid zone, and some species occur at a depth of 200 fathoms or more. The Alcyonaria have an equally wide range with the Caryophyllidæ, and probably reach still farther towards the poles. The Hydroidea range from the equator to the polar regions, but are most abundant in the waters of the temperate zone.

Besides the above mentioned limiting causes, there are others of importance, one of which may be alluded to in this place; the remaining, belonging more properly to the Geological Report on Coral Reefs and Islands, will be particularly considered in the forthcoming volume by the author. The

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* Lobophyllia of Blainville, Mussa of Oken.

† The evidences on this point will be presented in the Report on Coral Islands.

‡ The exceptions belong mostly to the genus Euphyllia, which includes the genus Flabellum, some Turbinaliæ, and the Lobophyllia, having entire lamellæ.

§ The *Caryophylliæ of Blainville*, with the *Dendrophylliæ*, Oculinæ, &c.

cause referred to is that proceeding from original sites or centres of distribution. There is sufficient evidence that such centres of distribution, as regards zoophytes, are to be recognised, The species of corals in the West Indies are, in many respects, peculiar, and not one can with certainty be identified with any of the East Indies. The central parts of the Pacific Ocean appear to be almost as peculiar in the corals they afford. But few from the Feejees have been found to be identical with those of the Indian Ocean. A more complete acquaintance with the corals of these different seas, will undoubtedly multiply the number of identical species; but observations thus far made, seem sufficient to establish as a fact, that a large part of zoophytes are confined to a small longitudinal range. This will be seen from the following table, exhibiting, in a general manner, as far as known, their geographical distribution. Each column gives the number *peculiar* to the region specified at top:—

	East Indies, Indian Ocean, or Red Sea.	Pacific Ocean.	West Indies.	Pacific, and the East Indies or Indian Ocean.	Extra-tropical.	Doubtful.	Total.
TRIBE ASTRÆACEA.							
Fam. Astræidæ, . .	37	50	29	4	3	16	139
Fungidæ, . .	14	29	6	6	0	10	65
TRIBE CARYOPHILLACEA.							
Fam. Caryophyllidæ,	13	7	9	2	13	5	49
Gemmiporidæ, .	4	5	1	2	0	2	14
TRIBE MADREPORACEA.							
Fam. Madreporidæ, .	30	42	4	8	1	7	92
Favositidæ,* .	14	15	5	3	0	4	41
Poritidæ† . .	5	14	6	2	0	1	28
	117	162	60	27	17	45	428

*** The Pocilloporæ, Sideroporæ, Milleporæ, Favosites, and other genera of Madreporacea, in which the cells are internally divided by horizontal septa.**

† Part of the Porites of authors, the species having shallow cells closed at

From this table it appears, that only twenty-seven species out of 306 are *known* to be common to the East Indies and Pacific Ocean. With regard to those common to the East and West Indies, for which no column is arranged, there are but two,—the Meandrina labyrinthica and Astrea galaxea, about which much doubt remains.

We have no authority for accrediting to the West Indies any species of the genera Fungia, Pavonia, Herpetolithus, Merulina, Monticularia, Gemmipora, Anthophyllum,* Pocillopora, Sideropora or Sericitopora, all of which are common in the opposite hemisphere. The Agariciæ, with the exception of two osculant species, are confined to the sub-genus Mycedia, exclusively West Indian, which contains very firm compact corals, often with an Astreæ-like character. The Millepores are the only known Favositidæ, and but half a dozen Madrepores have yet been distinguished. The Manicinæ, Caryophylliæ, and Oculinæ, are more numerous in the West Indies than elsewhere, and the Ctenophylliæ (Meandrinæ, with stout entire lamellæ), have been found only in the West Indies. The genus Porites contains several species, but they are uniformly more fragile and more porous than those I have seen from the Pacific and Indian Oceans; and the Polyps, as figured by Lesueur, are more exsertile, approaching in this particular, the Gonioporæ.—(*American Journal of Science and Arts, Second Series,* Vol. iii., No. 8, p. 160.)

Origin of the Hawaiian Nation.

All the Polynesians have clearly had one and the same parentage. Though their general resemblance in manners and customs, in religion and government, in appearance and dress, might be made to fill

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bottom (Porites clavaria, and the allied). The other Parites, with a few exceptions, belong to the genus Manopora of the author, and are true Madrepores in their cells, but with imperfect calicles or none; the *P. spumosa* of Lamarck, and the allied, are here included, besides the *Montipora* of Blainville.

* Sarcinula in part of Blainville, Caryophyllia in part of Lamarck, Anthophyllum of Schweigger, who introduces the name, but not of writers on fossil corals.

volumes, yet they would, one and all of them, be less conclusive on the point, than the fundamental correspondence, both in the words and in the structure of their languages. With but little difficulty, and in some of the instances with none at all, Tahitians, Marquesans, Samoans, Tongans, New Zealanders, and Hawaiians, to say nothing of the less known groups, can render themselves intelligible to each other; and of this similarity of dialects, the strongest as well as the most gratifying proof is to be found in the fact, that native converts of one Archipelago have sometimes gone forth, as missionaries, to communicate the glad tidings of salvation to another. Thus a chief who accompanied Mr Ellis from Tahiti to the Sandwich Islands, often addressed the natives with effect; and Sir Edward Belcher found a little colony of Samoan teachers labouring, or rather wishing to labour, among the savages of the New Hebrides. To offer more specific evidence of the fundamental correspondence in question, the subjoined table is quite decisive, at least with respect to the words; for the identical meanings of six nearly identical sounds, in three different dialects, cannot possibly be accidental.

Tahitian.	*Hawaiian.*	*Marquesan.*	*English.*
Ovai.	Owai.	Ovai.	Who.
Evau.	Ewalu.	Evau.	Eight.
Eiva.	Euva.	Eiva.	Nine.
Vahine.	Wahine.	Vahine.	Woman.
Maitai.	Maitai.	Maitai.	Good.
Tatou.	Tatou.	Tatou.	Tatoo.

Perhaps a careful examination of different dialects might suggest some hints as to their comparative antiquity. As the general tendency of language to become softer by change would derive special force from the feeble and almost childish organs of the race under consideration, any dialect might reasonably be deemed more recent, in proportion as its alphabet and pronunciation might be more meagre and effeminate. Now the common language of the Polynesian Isles appears to have travelled from the west towards the east. Thus the Hawaiians, and apparently the Tahitians also, abhor a concourse of consonants, while the New Hebrides have their Erromanga, the Feejees their Banga, and the Friendly Islands their Tonga, or, to use the better known name, their Tongataboo. If an Hawaiian were desired to pronounce any one of these three words, he would either insert a vowel between the two consonants, or omit the harsher consonant; and he would most probably adopt the latter course, just as he would transform England into Enelani. In all probability, *Tonga* and *Tona* or *Kona*, the name of a district, already mentioned, of Hawaii, are one and the same word; and, to give an instance of which there can be no doubt, *tangata*, the Samoan word for *man*, has been softened into the Hawaiian *tanata* or *kanaka*. Again, the very name of Samoa, the chief of the Navigator Isles, involves the letter *s*, which the Hawaiians, as also I believe the Tahitians, altogether reject, as being too much for their powers of utterance. Thus they change ***fashions*** into *patena* or *pakena; missionary* into *mitinary* or *mikinary;* and *consul* into *tonatele* or *konakele.* Finally, the Marquesan or Tahitian dialects, though they partake, in an eminent degree, of the softness of the Hawaiian, have yet retained at least one consonant, namely *f*, which it has discarded. The *Fatuiva*, one of the Marquesan Isles and *Paofai*, a chief of Tahiti, would, in the mouth of an Hawaiian, respectively become *Patuiwa* and *Paopai;* while there can be no mistake as to the original orthography, inasmuch as the *f* is distinguished in the one word from *v*, and in the other from *p*. Might not a similar application be made of the table which preceded this paragraph, with respect to these three dialects? In the first four of its six words, the *v* of the Tahitians and Marquesans becomes the *w* of the Hawaiians;—the former being, of course, a consonant; but the latter, however it may be classed by grammarians, being really *oo*, sounded as quickly as possible. If there be any truth in these desultory and incomplete suggestions, then must this Archipelago have been peopled after the Marquesas and the Society Islands, and they again after the more westerly group.

This result, which, so far as the Sandwich Islands is concerned, agrees with the traditionary lore of the Archipelago, is consistent with nearly all the arguments which can be brought to bear on the subject. Looking on the map at the tolerably continuous chain of islands and groups of islands, from Sumatra to the Marquesas, and at the comparatively open ocean between this its last link and the American continent, a plain man would instinctively infer, at least in the absence of evidence to the contrary, that Polynesia, as certainly as Australasia itself, must have been peopled, not from the new world, but from the old; and he would find his inference materially confirmed by the fact, that, on any and every hypothesis, the isles of the Pacific could have been colonized from the westward long before the eastern shores of that ocean contained a single family of human beings; while, on further investigation, he would confessedly discover vastly more numerous traces of Asia than of America, in the ethnographic characteristics of the Polynesian Isles.

The single, absolutely the only, answer to all this, is the physical fact, that the trade-wind blows from the east along the whole breadth of the route which has been chalked out for the primeval colonists of the islets of this greatest of all seas. Now, in the face of so much direct proof of an Asiatic origin, the evidence in question of an American origin amounts to nothing, unless the difficulty of advancing from west to east in spite of the trade-wind actually amounts to an impossibility.

But, so far from amounting to an impossibility, the difficulty itself, strictly so called, can hardly be said to have existed. As the trades, even at their *steadiest,* take to themselves a few points of elbow room, having ranged, for instance, in our own case, as already mentioned, between NE. and ESE., the Polynesian groups, occupying about fifty degrees of latitude, might all be intersected, without any formidable interventions of a foul wind, by one and the same track, starting from the westward; and, even independently of this constant oscillation of the ordinary current of the air, the same result could be still more easily and more directly attained with the aid of the opposite monsoons, which blow, with greater or less regularity, during two or three successive months of the year. Moreover, on such a point, one fact is more conclusive than a score of arguments; and, unfortunately for the partisans of the east wind, all the facts are stubborn supporters of the other side of the question. The inhabitants of each group, in whatever direction their ancestors reached it, think nothing of sailing from its westerly to its easterly islands; and Captain Beechy fell in with several men and women, who had drifted six hundred miles with a large canoe in the very teeth of the general direction of the prevailing trades. But, even if the alleged difficulty amounted to an actual impossibility, the claims of Asia to be the cradle of the Polynesians, though they might be weakened, would yet not be disproved. The westerly gales, which generally blow on either side of the region of the trades, might carry vessels far enough to the eastward, to make the tropical breeze a fair wind to the westward, more particularly if they had started from the more northerly coasts of Asia; and, in fact, one Japanese junk, in December 1832, was driven to Woahoo, with four men alive out of her crew of nine; while, again, in 1839, another was found drifting about half way on the same involuntary voyage, with several individuals on board, the same whom we afterwards saw at Ochotsk, which they had reached immediately from Kamschatka, on their homeward route from the Sandwich Islands.

Farther, if the trade-winds had really rendered a voyage from west to east impracticable, Polynesia would, in all probability, never have been peopled. There is not the least evidence for believing,—there is not the slightest reason for supposing,—that the Aborigines of America ever possessed a canoe or any other vessel stout enough to survive the dangers of the intervening ocean, during a voyage which could not, under the most favourable circumstances, occupy less than three or four weeks. All the obstacles of the trade-wind notwithstanding, I should more readily conclude, that the Marquesas colonized Southern America, than that Southern America colonized the Marquesas,—so far, at least, as the mere question of navigation might be concerned.

From what country, then, of Asia, did the Polynesians spring? Almost, to a moral certainty, from some point, or rather points, between the southern extremity of Malacca, and the northern limits of Japan,—an answer which appears to be corroborated by that most conclusive of all features of resemblance, the similarity of language.

Premising that, in such a case, nothing like identity is necessarily to be expected,—for, according to general experience, the human race was diffused over the globe rather by the migration of whole tribes, than by the emigration of parts of them,—there seems to be no ground for doubting, that the dialects of Polynesia are connected with the languages of the adjacent coasts of Asia. To say nothing of the admitted fact, that the Chinese residents of the Sandwich Islands pick up the Hawaiian with great facility in a short time, the Malayan tongue is universally allowed to be a striking analogy to the language of the groups of the Pacific. To the eye indeed, and perhaps also to the ear, there is said to be a staggering difference in the predominance of vowels on the part of the latter, and of consonants on the part of the former. This difference, however, is susceptible of a satisfactory explanation. The concourse of consonants in the Malayan arises, in a great measure, from an admixture of the Arabic, which, to a moral certainty, must have taken place long after Polynesia began to be peopled; and, even if the admixture in question had been anterior to the colonization of any of the islands, the concourse of consonants just mentioned would, to a considerable extent, have been nominal, inasmuch as the short vowels of the Arabic are rounded without being written. But, further, the peculiarity under consideration of the language of Malacca, supposing it to have been both original and real, would tend rather to support than to impugn the foregoing views. The Hawaiian has been shewn to embody fewer consonants than the Marquesan or the Tahitian, and the Tahitian and the Marquesan again to embody fewer than the Samoan, to the Fejeean, or the Tongan, or the dialect of the New Hebrides, the *taboo* of the eastern groups, to add another instance to the instances already cited, assuming the form of *tamboo* to the westward. Now, on the very same principle, one ought not to be surprised to find that the consonants become more numerous and more harsh as one approaches to the native seats of a language so widely diffused.

To conclude this head with one remark more,—if any ethnographic similitudes do exist between America and Polynesia, they may be safely considered as common results of one and the same cause. Though the new world must have received inhabitants from the old across the strait which separates them, just as certainly as if the two were connected by an isthmus, yet it might, in all probability, have received others, and those too, in more regular and continuous streams, along the chain of stepping-stones, which extend from China to the north-west coast, comprehending Japan, the Kurile Islands, and the Aleutian Archipelagoes; and, to shew that this supposition is far within the limits both of possibility and of probability, a Japanese junk, such as has been used since the first settlement of this country, lately found its way to the western shores of the new continent, with a living crew on board, and without the aid of any intermediate place of refreshment or of rest. In a word, America and Polynesia appear to have been chiefly, if no solely, colonized from one and the same general region of Eastern Asia.—*Narrative of a Journey round the World, by Sir George Simpson*, vol. ii. p. 2.

On the Age of the Volcanoes of Auvergne as determined by the Remains of successive Groups of Land Quadrupeds. By C. LYELL, Esq., F.R.S., &c.*

The region of extinct volcanoes of Auvergne derives its peculiar interest from the circumstance of its never having been submerged beneath the sea during a period in which its geological and geographical structure, and the animals and plants by which it has been inhabited, have undergone a great succession of changes. In the rest of Europe, generally, the volcanic rocks have either been originally of submarine origin, or the surface, since they were produced, has suffered so much denudation by the action of the waves of the ocean as to make it impossible for us to ascertain the form and manner in which the eruptions took place, or the relative position which the igneous formations held at first to the hills, plains, and valleys then existing. After describing the several classes of rocks in Auvergne,—the granite, the eocene fresh water, and the older and modern volcanic, each depicted by different colours in an extensive landscape, enlarged from a view of the valley of Chambon (Puy de Dome), by Mr P. Scrope,—Mr Lyell said he should dwell chiefly on the antiquity to be ascribed to the Puy de Tartaret, a type of one of the most modern cones of eruption in Central France. The comparatively recent origin of this conical hill of scoriæ, with its crater at the summit, is proved by its standing at the bottom of a deep valley excavated through the alternating beds of pumice, trachyte, and basalt, belonging to the more ancient volcano of Mont Dor, and partly through the subjacent and fundamental granite. It is farther confirmed by the course of a powerful current of lava; which, proceeding from the base of the cone, flows thirteen miles down the channel of the river Couze, stopping at the town of Nethers, near Issoire. The lava occupies the ancient river bed, and is observed to contract in its dimensions in the narrow gorges, where it also gains in height, like the water of a river flowing through the arch of a bridge; and to expand again where the valley opens, where it speads into a broad sheet having a level surface. It also flows up the channels of tributary streams till it attains a level corresponding with the top of the lava at the point of junction of the tributary with the main valley. But although these appearances prove that the lava has flowed as it would now do if it were remelted and made again to descend the same channel, it nevertheless bears, in some parts of its course the marks of considerable age.

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* Account of a Lecture read before the Royal Institution on April 20, 1847, by Mr Lyell.—*Athenæum*.

Before considering these, Mr Lyell entered into a short digression to refute the doctrine of the mediæval origin of the volcanoes near Clermont, advanced by a writer in the *Quarterly Review* for October 1844 (p. 295), where it is pretended that Sidonius Apollinaris, Bishop of Clermont, who flourished at the close of the fifth century, has borne explicit testimony to "the volcanic eruption, the crumbling of the cones, and the heaping up of the showers of ashes and scoria cast forth amidst their fires." The passages relied on, occur in a letter from Sidonius to his contemporary, Mamertus, Bishop of Vienne, in Dauphiny, written when Auvergne was threatened with a fresh eruption of the Goths; to avert which danger the Bishop proposes to adopt certain forms of prayer (rogations or litanies), which Mamertus had already introduced on the occasion of some "prodigies" which had happened in Dauphiny sixteen years before. In alluding to these phenomena, Sidonius says that "the walls of the city of Vienne were shaken by frequent earthquakes, many fires broke out, and mounds of ashes were heaped up over the fallen copings of the walls." "Nam modo scenæ mœnium publicorum crebris terræ motibus concutiebantur, nunc ignes sæpe flammati caducas culminum cristas, superjecto favillarum monte tumulabant." Deer also took refuge in the forum, and the people fled; all but the Bishop, who had a right to reckon on Divine protection, because, as Sidonius reminds him, on a former occasion, the flames at his approach had miraculously receded, out of reverence to his holy person. At the time of the earthquake he (Mamertus) had told his people that their repentant tears would extinguish the fires sooner than rivers of water, and the steadfastness of their faith would cause the rocking of the ground to cease. Sidonius finishes with asking the Bishop of Vienne to send him some relics to make all secure. The style of the whole epistle is so faulty, ambitious, and poetical, as to make it difficult to know the exact value of the expressions, and dangerous to found upon them any philosophical argument about natural events. There is not a word about Auvergne, but simply an allusion to the shocks which appear to have thrown down buildings and caused (as usual in such cases where roofs fall in) great conflagration and heaps of cinders. The terror of the wild animals when the earth rocks, and their sensitiveness to the slightest movements, are well known. Although the epistle proves Sidonius to have had a fair share of the credulity of his age, both in respect to miracles wrought in favour of a contemporary saint and the efficacy of relics, it would be unfair to charge him with a belief in the occurrence of a volcanic eruption at or near the site of the city of Vienne, which the investigation of the ablest government surveyors, to whom the construction of a geological map of France has been intrusted, has entirely disproved. There are, in fact, no monuments of volcanoes, ancient or modern, in Dauphiny; and if there had been, they would not throw light on the date of eruptions in Auvergne.

But to return to the lava-stream of the Puy de Tartaret before alluded to—what geological antiquity can we assign to it? In one of the gorges, the entire mass of solid basalt has been swept away by the torrent, so that the former continuity of the stony current is interrupted for several hundred yards, at a point about midway between its efflux from the cone and its termination. This implies a long period of excavation. In another place, about one mile and a half from St Nectaire, an old Roman bridge, still passable, having two arches, each fourteen feet wide, spans a deep ravine, cut by the Couze through the middle of the lava, which is here of columnar structure. The bridge is supposed by French architects and antiquaries, to be of the date of about the fifth century; yet the springing of the arches proves that, when it was erected, the ravine was the same width as now. Nevertheless, while signs of denudation, such as these, attest the vast amount of removal of hard rock since the lava flowed and was consolidated, the contemporary cone of loose, incoherent scoriæ has stood in its exposed position at the very bottom of a valley, entire and uninjured, the rain-water being instantly absorbed by the porous mass; and no rill being allowed to collect on its flanks. It is clear that if any flood of water had passed over Auvergne, if any inundation had raised the Lake of Chambon 30 or 40 feet, it must have carried away the perishable cone. The lake alluded to owes its origin to the damming up of the Couze by the volcano, and by landslips which accompanied the eruption.

But the most conclusive evidence, according to Mr Lyell, of the remoteness of the period at which the cone and lava of Tartaret originated, has yet to be set forth, and has only been distinctly brought to light since he revisited Nechers in 1843, when the Abbé Croizet pointed out to him a locality near the lower extremity of the great current, where fossil bones of extinct animals had been discovered in a meadow, between the base of the lava and the channel of the Couze, now 10 feet lower in level than the lava. In company with Mr Bravard, Mr Lyell explored the spot; and they convinced themselves that the bone-deposit passed under the lava, which here forms a mass 30 feet thick. Subsequent investigations not only confirm this view, but have enabled Mr Bravard to obtain from beneath the stony current a considerable number of additional osseous remains, referable to the genera Equus, Sus, Tarandus, Cervus, Canis, Felis, Martes, Putorius, Sorex, Talpa, Arvicola, Spermophilus, Lagomys, Lepus, and, according to Mr Waterhouse, Cricetus or hamster, and others, besides the remains of a frog, lizard, and snake, and the bones of several birds. Mr Owen has examined some of these remains for Mr Lyell, and recognizes among them the *Equus fossilis* and *Tarandus priscus*, both extinct species, occurring in the caves of England, with the contents of which generally this assemblage of fossils from Auvergne appears to agree very closely—there being a predominance, according to Messrs Croizet, Bravard, and Pomel, of species not known to exist at present, with an intermixture of a few others undistinguishable from quadrupeds now inhabiting Europe. Among the landshells associated with the bones, were found *Cyclostoma elegans*, *Clausilia rugosa*, *Helix hortensis*, *H. nemoralis*, *H. lapicida* and *H. obvoluta*—all recent, and all, with the exception of the last, now found in the immediate neighbourhood. Mr Lyell thinks it probable that the deposit of red argillaceous sand under the lava containing these remains, was derived chiefly from volcanic matter, which the eruption of Tartaret threw out, and that the fossil animals perished by floods occasioned by that outburst. That a similar fauna continued to live in Auvergne after the latest eruptions, is inferred from the discovery of the remains of many of the same group of animals—spermophilus, lepus, castor and others, in the clefts of a lava-current as modern as that of Tartaret, observed at Aubier, near Clermont. This fauna, so different as a whole from that now living in Europe, evidently inhabited Auvergne when the valley of the Couze had been excavated down to the same level as that over which the lava of Tartaret flowed: yet its antiquity must be extremely great—the gradual dying-out of species, and the introduction of new ones taking place, according to Mr Lyell's views, with extreme slowness. The fact that the shells belonged *all* to living species (which possibly might not hold good if a larger number were obtained) affords no presumption against an indefinitely remote origin, as compared to the periods of history and tradition, because the lecturer has shewn that the ravine of the Niagara ("Travels in N. America," vol. i. ch. 2), and the Delta of the Mississippi (Reports of the Brit. Assoc. for 1846), both of which must have required an enormous period for their formation, are, nevertheless, posterior in date to deposits full of the recent land and fresh-water shells of North America, associated with the remains of quadrupeds, nearly all of which are now extinct.

It was next shewn that all the volcanoes of the modern class of which the Puy de Tartaret is a type, were not formed at once, for the lavas of some (as for example, at Champheix, in the same valley of the Couze) stand at a greater height above the actual river-courses, and repose on ancient alluvium formed when the valleys were shallower. To allow time for the ejection of these numerous cones and lava-currents, of which there are several hundreds in Central France. we require a long series of ages, all subsequent to the miocene period, to which another class of monuments of anterior date are referable—as, for example, the bone-bearing alluviums alternating with volcanic formations (pumiceous and trachytic) of Mont Perrier, to which a distinct fauna (of the genera mastodon, elephant, hippopotamus, tapir, &c.) belongs. Some of the valleys cut out of the still more ancient lacustrine strata were only half eroded to their present depth in the miocene period, and were occasionally filled up with miocene deposits, and afterward re-excavated. It is possible in Auvergne to distinguish the relative ages of a great variety of alluviums containing the bones of terrestrial quadrupeds, in consequence partly of their preservation under lavas of different ages, and partly their position on the sides of valleys which were gradually deepened; no flood or return of the ocean having disturbed the surface and mingled the fossils of one period with those of another, as has happened in England and most parts of Europe. The oldest fauna of land quadrupeds in Auvergne,—that found in a fossil state in fresh-water strata of marl and limestone, older than the trachyte of Mont Dor,—consisted of species of Paleotherium, Anoplotherium, Anthracotherium, Opossum, &c., analogous, in great part, to those of the Paris basin, with some miocene forms associated, and belonging, according to Mr Lyell, to an upper eocene group, newer than the Parisian tertiaries, or the uppermost fresh-water of the Isle of Wight. Hence it follows that the whole succession of revolutions in the animate and inanimate creation which have occurred in Central France since the land emerged, vast as they are in duration, as compared to the era of the more modern volcanoes, is, nevertheless, considerably posterior to the marine clay on which London is built;—this last being one of those tertiary deposits which rank as but the monuments of yesterday in the great calendar of geological chronology.

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On the Ancient Extent of the Glaciers of Chamonix from Mount Blanc to the Jura. By M. CH. MARTINS.

In the month of August 1815, a geologist returned from a long excursion to the glaciers which occupy the bottom of the valley of Lourtier, a valley lying in a lateral direction to that leading to the Great St Bernard. Wishing to reach the hospice on the following day, by a difficult and little known pass, he spent the night in the hut of a chamois-hunter, named Jean-Pierre Perraudin, who was to be his guide next day. Seated before a fire formed of branches of rhododendron, the odoriferous smoke of which escaped from the top of the roof, the geologist and the mountaineer spoke of the elevated region which both of them had so often traversed. The conversation then fell on those large granite blocks often found at such great distances from the rock from which they have been detached. The geologist explained at length to the mountaineer how men of science had shewn, by means of profound calculations, that the erratic blocks had been formerly transported by strong currents of water. To all this Jean Perraudin could answer nothing, but he shook his head with an air of doubt and incredulity. "I am of opinion," he said at length, "that the glaciers of our Alps were much more extensive formerly than in the present day. The whole of our valley, to a great height above the torrent of the Drance has been filled by a vast glacier which descended as far as Martigny, as is proved by blocks of rock found in the neighbourhood of that town, and which are far too large to be transported by water." In speaking thus, Perraudin little suspected that he had made a great discovery, and solved, by his own good sense, a problem which the genius of the most celebrated geologists, furnished with all the resources of science, had attempted without success.

Fortunately the individual to whom he thus communicated the result of his solitary observations was a practical man, more interested in facts than theories. The hint the peasant suggested to his mind developed itself freely, and the idea of the ancient extension of glaciers beyond their present limits, became, for twenty years, the constant object of his researches and reflections. One of his friends, an engineer, M. Venetz, had been led to entertain the same views from studying the erratic blocks of the Valais. Lastly, in 1834, when his conviction was complete, and supported by numerous and irresistible proofs, M. de Charpentier (for it was he who had been Perraudin's confidant) brought forward his opinion at the meeting of Swiss naturalists held at Lucerne. Like every new idea, it was received with coldness, or treated with disdain; but, as it was founded on truth, it made progress of itself, and has now become one of the most important questions which have engaged the geological public. Owing to the great number of works published of late years on this question,* the phenomenon of the Alps has assumed the importance of a great revolution, and its theatre may be said to include a considerable portion of the two hemispheres. If human genius should one day explain the cause of this glacial cataclysm, it will throw the brightest light on the last phase of the geological history of the globe,—on the mysterious epoch which preceded the appearance of Man on the surface of the earth,—and on that universal Deluge, of which traces are found in the traditions of nations in Europe, Asia, and the two Americas. The intimate relation which connects these two phenomena cannot be denied, for they are attested at once by reason and observation. Yet we do not propose to pursue the study of glacial phenomena in all the countries where they have been attended to; we confine ourselves to the study of them among the Alps, where the facts, well known and appreciated, may be verified every year by numerous travellers.

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* Among these works, we may mention those of MM. Agassiz, Desor, A. Guyot, J. Forbes, Studer, A. Escher de la Linth, and Blanchet, relating to the Alps; Leblanc, Renoir, Hogard, and E. Collomb, on the Vosges; Agassiz, Lyell, Buckland, Smith, Maclaren, in Scotland, England, and Ireland; M.

Have the glaciers of Switzerland and Savoy always been confined within their present limits, or did they formerly extend into the great plains which lie around the chain of the Alps? Such is the problem, when reduced to a simple form. My object is to explain the facts on which the advocates of the ancient extension of glaciers rest the argument. In order to establish their views, they have to combat, among philosophers, convictions of old standing, resting on the most unexceptionable authorities in geology; among men of the world, the testimony of biblical tradition, and that of all the senses, which revolt against the idea of considering these plains, now so fertile and animated, as having been buried during long periods under an immense sheet of snow and ice. Both parties have a right to demand numerous and positive proofs. These proofs exist; but, before examining them, it is necessary to possess some knowledge of existing glaciers; for the method followed by geologists, to whom we are indebted for the results which we are about to lay before the reader, has always been that introduced into science by M. Constant Prevost, and which may be briefly expressed in this

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Brongniart, Sefstroem, Keilhau, Boethling, Siljestroem, Daubrée, Murchison, Verneuil, and Durocher, in Scandinavia; Hitchcock and Darwin, in America.

way: " To study the mode of action of natural elements which we witness before our eyes, and compare the effects they produce with those of which traces are still preserved on the surface of the globe." By proceeding on this principle, we shall find that we can discover everywhere, in the vast plains which surround the Alps, traces of those gigantic glaciers of which such as now remain may be said to be the mere miniatures. Still, although reduced to small dimensions, existing glaciers present us, on a small scale, with all the phenomena that formerly occurred in fields of ice on a large scale. The effects are the same, and we can infer their identity from that of the agents which have produced them.

1. *Of Existing Glaciers.*

From the summit of the ridge of the Jura, which overlooks the Leman basin, we obtain a bird's-eye view of the whole chain of the Alps, from the Valais to Dauphiny. The colossal mass of Mont Blanc, resting on its broad base, rises majestically and alone above this long serrated ridge. The highest peaks are distinguished from the less elevated summits, by the sparkling whiteness of the snow which covers them. In summer, the lower limit of these perpetual snows forms a straight horizontal line, perfectly marked, which contrasts with the sombre verdure of the forests along the base of the mountains. This line is that of eternal snows. Above it, winter reigns alone; below it, the seasons follow their regular course. Above it, life scarcely exists, and is represented only by a few polar plants, and some ephemeral insects; below, it is manifested in a thousand varied forms, from the highest regions where the pine and the chamois venture to appear, down to the plains inhabited by man, where the harvests become yellow, and the vine ripens its fruit.

In Switzerland, the lower limit of perpetual snow is about 2700 metres above the level of the sea; but as we approach the Alps, penetrating into the narrow valleys which intersect the principal masses, such as those of Mont Blanc, Mont Rosa, St Gothard, and the Jungfrau, we perceive that this limit is not a straight line, as it appeared when seen at a distance. The fields of perpetual snow emit branches, so to

speak, which descend into the valleys in the for
of ice, resembling frozen torrents. These masses
Their lower extremity is often more than 1500 n
the limit of perpetual snow, and sometimes app
vicinity of large villages, such as those of Char
mayeur, and Grindelwald, the mean height of wh
1120 metres above the sea. At the same time, a
ber of glaciers exist which do not descend so l
on elevated slopes, where we find only a few sc
lets, inhabited only a few months in the year.

What are the relations that exist between th
and the fields of snow with which they are conne
first question we have to examine. Science has
swered it. In winter, spring, and autumn,
masses of snow fall on the summits of the Al
snows, driven about by the winds and whirlwin
late, particularly in the great depressions in th
the high peaks. These depressions are known
of amphitheatres or *cirques*, for they commonl
in a semicircular enclosure, surrounded with ele
mits. Of this description, in the environs of Ch
the cirque which terminates at the Col du Geant
plateau, which is only 800 metres below the pe
Blanc; near Grindelwald, the amphitheatre whi
the Strahleck; at the Grimsel, those of the Lau
Finsteraar. The snows that accumulate in these
not remain immoveable; they have a progressi
which makes them tend towards the valley. Lik
which feed a river, and whose waters begin to flo
soon as the influence of the declivity is felt, the
can slide over ground but very slightly in
tion as this snow descends into the
it undergoes, particularly in the
fications, which complete

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* The height of the
level, was 16 metres 6
1846. The sheet
metre 4 decemetr

nged into ice. This transformation takes place in the
wing manner. Under the heat of the sun's rays, the
ace of the snow begins to melt; the water thus produced
trates into the lower beds, which are changed, under the
ence of the night frosts, into a granular mass, composed
mall flakes still in a state of disaggregation, but more ad-
ent than the flakes that produced them. This state of
snow has been named by the Swiss Natural Philosopher
. Throughout the whole summer, this névé becomes
trated with additional quantities of water, always pro-
ling from the superficial melting, or from that of the
hbouring snows, whose waters have united in the de-
sion which forms the cradle of the glacier. In these re-
s, the thermometer falling every night below zero, even
he middle of summer, the névé is frozen by many succes-
congelations. In consequence of these successive melt-
and congelations, it presents the appearance of a white
pact ice, but filled with an infinity of small spherical or
roidal bubbles of air; this is the *vescicular ice* (*glace bul-
e*) of the authors who have written on this subject. The
tration and congelation of the mass becoming more and
e perfect, as the glacier descends towards the inhabited
ons, water at length replaces all the air-bubbles; the
sformation is then complete; the ice appears homogene-
and presents those beautiful azure tints which have ex-
d the admiration of travellers. Such, in a few words, is
history of the formation of a glacier; in reality, it is com-
d, as is seen, of all the layers of snow accumulated dur-
long series of years, and which is converted by degrees
ice, more or less compact.

the summer heats did not set a limit to the increasing
iers, they would go on enlarging indefinitely in length
momentum; but every summer causes a considerable
ease in the thickness of the glacial surface.* This is the
omenon which M. Agassiz has designated by the name
lation. At the same time, the lower extremity melts
lly, and the glacier would diminish every year, if an in-

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* *Nearly three metres.*

cessant progression did not counterbalance this effect. A kind of equilibrium is thus established between the summer melting on the one hand, and the annual progression on the other. If the season be warm and dry, the fusion predominates, and the glacier recedes; if the summer be cold and rainy, the progression more than compensates the effects of the fusion, and the glacier advances.

We now understand what are the influences which assign to glaciers a mean limit, around which they may oscillate without ever surpassing it. It is less easy to give a reason for certain glaciers descending into inhabited valleys, while others remain suspended on the sides of the highest mountains. These differences are owing to the size and height of the amphitheatres or cirques which feed these glaciers. The more extensive and elevated these cirques are, the more considerable will be the quantity of snow which accumulates on them, and the more also will the outshoots from the fields of snow descend into the low valleys, and regain, so to speak, the ground which had been lost by the annual melting. It is in this way that the glacier of the Bossons, whose source is in the great plateau of Mont Blanc, a vast amphitheatre, situate nearly 4000 metres above the sea, descends to 1040 metres, and advances to the midst of inhabited places, orchards, and cultivated fields. The glaciers of Aletsch, Viesch, Grindelwald, and Zermatt, are in the same state. Every year, the astonished traveller may see golden harvests by the side of the glacier of Brenva, which descends the southern face of Mont Blanc. The influence of the size and elevation of the amphitheatres even counterbalances, according to M. Desor's remark, that of exposure; and explains the surprising fact, that the longest and most important glaciers of the Bernese Alps are found on the southern declivity of the chain.

We have seen that these glaciers have a progressive movement which draws them towards the plain. What are the laws which regulate this movement? The investigation of these laws has continually occupied the attention of all the natural philosophers who have devoted themselves to this subject, yet they have not hitherto deduced the cause of this advance from the whole of the singular phenomena which acterise it. Mr J. D. Forbes studied them on the Mer lace of Chamonix; but it is on the glaciers of the Aar the observations have been prosecuted with most care perseverance. From 1842, MM. Agassiz and Desor, as- d by MM. Wyld, Otz, and Dollfus-Ausset, have been un- ingly occupied with this question; they have ascertained , in its medium part, this glacier advances 71 metres a . Toward the lower extremity, the rapidity of the pro- sion decreases till it does not exceed 39 metres: it accele- s a little, on the contrary, towards the top, where the ier annually traverses a space of 75 metres.*

he inclination of the slope on which the glacier descends not appear to have any influence on the rapidity of its ress, but it is singularly modified by the walls of the hol- in which it moves. The friction of the ice against these s considerably retards the advance of the lateral parts of glacier. Besides this, if a promontory jut out towards the lle of the valley, the glacier, arrested on one of its sides, s round the obstacle with extreme slowness, or rather this remains behind, while the central portion and the oppo- edge continue to advance with their relative quickness.

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he following is a short account of the method in which the advance of a r was measured. On the two banks, two rocks were chosen opposite each ; each of these rocks was marked by a white cross painted upon the stone. ies of stakes were then fixed in the ice in a line between these two points, to form a straight line perpendicular to the axis of the glacier. After days, an observer placed himself before one of the crosses, and directed scope, bearing a level, towards the one opposite. The glacier having ced, and the stakes along with it, the latter were no longer in their al line. Then a guide placed on the glacier and carrying a pole sur- ted by a very visible object, placed the pole in the direction of the former This direction was indicated to him by the signal of the observer, whose as applied to the telescope. The latter caused the pole to be carried back was exactly at the point formerly occupied by the line of stakes. This the guide measured the distance on the ice from the foot of the pole to f the stakes. This interval was exactly the length traversed by the glacier en the observations. This year, the process has been modified by MM. as, Otz, and myself, in such a manner as to permit us to follow the daily ess of the glacier of the Aar with such an exactness, that the error of ob- tion cannot exceed two millimetres, or about a line.

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The inclination of the slope on which the glacier descends does not appear to have any influence on the rapidity of its progress, but it is singularly modified by the walls of the hollow in which it moves. The friction of the ice against these walls considerably retards the advance of the lateral parts of the glacier. Besides this, if a promontory jut out towards the middle of the valley, the glacier, arrested on one of its sides, turns round the obstacle with extreme slowness, or rather this side remains behind, while the central portion and the opposite edge continue to advance with their relative quickness.

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2. *Rocks Polished and Striated by existing Glaciers.*

The friction of the glacier on its bottom and walls is too considerable not to leave traces on the rocks with which it comes in contact; but its action is different according to the mineralogical nature of these rocks, and the configuration of the bed it occupies. If we penetrate between the ground and the lower surface of the glacier, taking advantage of the caverns of ice which sometimes open on its sides and extremity, we creep on a bed of pebbles and fine sand mixed with water. If we remove this layer, we find that the subjacent rock is levelled, polished, worn by the friction, and covered with rectilinear striæ, sometimes resembling small furrows, more frequently perfectly straight rays which have been cut by a graving tool, or even a very fine needle. The mechanism by which these striæ have been engraved is the same that human industry employs to polish stones and metals. By means of a fine powder called *emery* we rub the metallic surface, and give it a brilliancy which arises from the reflection of the light from an infinite number of excessively fine striæ. The layer of pebbles and mud interposed between the glacier and the subjacent rock, represents the emery. The rock is the metallic surface, and the mass of the glacier, which presses and displaces the bed of mud by continually descending towards the plain, represents the action of the polisher's hand. Accordingly the striæ of which we speak are always in the same direction as the progress of the glacier; but as the latter is liable to small lateral deviation, the striæ sometimes cross each other, and form with each other very small angles. If we examine the rocks which border the glacier, we find the same striæ engraven on the parts which have been in contact with the congealed mass. I have often taken pleasure in breaking the ice which pressed upon the rock, and under the ice I found the surfaces polished and covered with striæ. The pebbles and grains of sand which had engraved them were still fixed in the glacier, as the diamond of the glazier is fixed at the extremity of the instrument which he uses to cut glass.

The distinctness and depth of the striæ depend on many circumstances. If the fixed rock be calcareous, and the emery composed of pebbles or sand derived from harder rocks, such as gneiss, granite, or protogine, the striæ will be very strong marked. This may be observed at the foot of the glaciers of Rosenlaui and Grindelwald, in the Canton of Berne. On the contrary, if the rock be gneiss, granite, or serpentine, that is to say, very hard, the striæ will be shallower and less marked, as may be observed in the glaciers of the Aar, Zermatt, and Chamonix. The polish will be the same in both cases; and it is often as perfect as that of the marbles which ornament our buildings.

The striæ engraved on the rocks containing these glaciers, are in general horizontal or parallel to the surface; but at the narrowing of the valleys these striæ rise and approach the vertical. We need not be surprised at this. Forced to make its way through a narrow pass, the glacier rises at the sides, and ascends along the flanks of the mountains which obstruct its progress. This is admirably witnessed near the Chalets of Stieregg, a narrow defile through which the lower glacier of Grindelwald is obliged to force its way, before it spreads itself in the valley of the same name. On the right bank of this glacier, the striæ are inclined 45° to the horizon; on the left bank, it rises sometimes to the neighbouring forests, and drags along large banks of earth covered with tufts of rhododendron, elder, birch, and pines. The soft and foliated rocks are bruised and broken in pieces by the prodigious force of the glacier. The hard rocks resist it; but these rocks also, by their flattened, worn, polished, and striated surfaces, bear testimony to the enormous pressure to which they have been subjected. It is in this way that, in the glacier of the Aar, the foot of the promontory on which M. Agassiz has erected his pavilion, is polished to a great height; and on the face turned up the valley I observed striæ incline 64°. The ice rising against this encampment seems as if it would have scaled it; but the granite rock resisted, and the glacier was obliged to turn slowly round it.

In short, the considerable pressure of a glacier, joined to its progressive movement, acts at the same time on the bottom and sides of the valley which it traverses. It polishes all the rocks which afford sufficient resistance not to be destroyed by it, and often impresses on them a particular and characteristic form. By destroying all the asperities of these rocks, they level the surface, and give them a rounded form in front, while, behind, they sometimes preserve their abrupt, unequal, and rugged forms. It will be understood that the effect of the glacier is exerted principally on the side turned towards the amphitheatre from which it descends, in the same manner as the piers of a bridge are more damaged in front than behind by the ice carried down the river in winter. Seen at a distance, a group of rocks rounded in this manner reminds the spectator of the appearance of a flock of sheep; hence the name *roches moutonnées* given them by Saussure, and which has continued to be applied to them.

3. *Moraines and Erratic Blocks of existing Glaciers.*

There is another class of phenomena of great importance in the history of existing glaciers, and of those which formerly covered Switzerland. I speak of the fragments of rock of every size and nature which a glacier carries along with it. The appearance of the Alps seems to intimate to us that they are immense ruins. Everything conspires for their destruction; all the elements seem combined to abase their haughty peaks. The masses of snow which rest upon them in the winter, the rain which filters into their strata in summer, the sudden action of the waters of torrents, and the slower, but still more powerful, influence of chemical affinities, degrade, disintegrate, and decompose, the hardest rocks. Their debris fall from the summits into the amphitheatres occupied by glaciers, in considerable masses, accompanied by a frightful noise, and large clouds of dust. Even in the middle of summer, I have seen these avalanches of stones precipitated from the top of the peaks of the Schreckhorn, and form on the spotless snow a long black track, composed of enormous blocks, and an immense number of smaller fragments. In spring, the rapid melting of the winter's snow often gives rise to accidental torrents of extreme violence. If the melting be slow, the water insinuates itself into the smallest fissures of the rock, becomes frozen there, and splits the most refractory masses.

The blocks detached from mountains are sometimes of gigantic dimensions, some are found 20 metres in length, and such as measure 10 metres in every direction are not rare in the Alps.

If the glacier were immoveable, these debris would remain fixed in it without any order, but its movement causes a certain arrangement in the distribution of these materials, which shews a very remarkable degree of regularity. These blocks are disposed on the glacier in long tracks parallel to its sides, or they accumulate at the extremity in the form of large transverse mounds. Both of these accumulations have been distinguished by the name of *moraines.*

The following is the mechanism employed in the formation of moraines. The debris of the surrounding mountains falling on the *sides* of the glacier, they share in its motion, and advance along with it; but, additional quantities of materials falling upon it, it may be said daily, are deposited in a line with the first, and the whole united form those long convoys of substances which lie along the two sides of the glacier; these are the *lateral moraines.* A glacier often presents many lateral moraines, because the debris from above fall at points unequally distant from the centre, and the degree of their motion is consequently different. The greater part of tourists who have visited the great glaciers of Switzerland are acquainted with these lateral moraines, and more than one of them can recall the fatigue they had to undergo before they could scale these accumulations of gigantic blocks. They might be called ramparts raised by giants to prevent access to these fields of eternal snow, where nature has concealed the secret of the last revolutions of our globe. After passing the lateral moraine, the traveller almost always discovers a mound still more considerable, disposed longitudinally towards the centre of the glacier, and which is named the *median moraine.* This results from the junction of two glaciers of nearly equal magnitude. At the extremity of the projection which separates them, the left lateral moraine of the one rests against the back of the right lateral moraine of the other. These two lateral moraines are soon confounded in one, and form the median moraine of the new glacier, itself composed of the two glaciers united. Thus, at the junction of the Arve and Rhone, we see the turbulent water of the torrent mingled at the central point of confluence with the transparent waters of the river purified by its passage across the lake of Geneva. The median moraine partakes of the motion of the medium part of the glacier; after a longer or shorter transit, each block reaches in its turn the terminal escarpment, rolled along the declivity till it be stopped at this rampart of ice. On the glacier of the Aar, the length of which is 8 kilometres, a block takes 133 years to traverse the space comprised between the promontory of Abschwung, which separates the two principal affluents and the lower extremity. The accumulation of these blocks forms a concentric mound at this extremity; this is the *terminal* or *frontal moraine*, which differs from all of which we have spoken in this, that it does not rest upon the glacier, but before it, on the bottom of the valley.

We are now acquainted with three kinds of moraines; some *superficial*, extended on the surface of the glacier, and which are divided into *lateral* and *median* moraines, according as they are on its sides or in the middle; and the *terminal* moraine, produced by the accumulation of blocks which form the terminal escarpment of the glacier, and rest upon the ground. There is still another kind of moraine, that is, the layer of sand and pebbles interposed between the lower surface of the glacier and the subjacent rock. I shall designate this *deep* moraine (*moraine profonde*) in order to distinguish it from such as are superficial and terminal.

4. *Pebbles Striated by existing Glaciers.*

Carried slowly along on the surface of a glacier, all the blocks of superficial and terminal moraines retain their original forms. The edges of these blocks are sharp, the angles acute, as at the moment when they fell upon the ice. They present no marks of wear or friction, such as we notice on stones rolled and rounded by the action of water. We may detach from them beautiful groups of crystals, as perfect as when in their original bed; for, except the first fall which precipitated them on the glacier, these masses have been subjected to no violence. Atmospheric agents alone can affect them; accordingly, the blocks composed of hard and resisting rocks often preserve the colossal dimensions of which we have spoken.

Such is not the case with the fragments which do not form part of the superficial moraines. The lateral walls of the glacier are not in immediate contact with the sides of the valley; there is almost always a small interval between them. Numbers of blocks and debris are entangled between this wall of ice, and the rocks which it polishes. Some of them remain suspended in this space; others, by degrees, sink to the lower surface of the glacier, and form the *deep* moraine. To these blocks are added, a part of those which fall into the numerous crevasses and wells,* objects which are so formidable to inexperienced travellers. All these debris, enclosed between the rock and the glacier, pressed, bruised, and broken by this great press continually in action, do not preserve the dimensions they had when detached from the mountains. The greater part of them are reduced to an impalpable mud, which, mingled with the water flowing from the glacier, forms the bed of mud on which it rests. Others preserve indelible traces of the pressure to which they have been subjected. All their angles are blunted, their sharp ridges effaced, and they assume the form of rounded pebbles, or present unequal facettes, resulting from prolonged friction. If the rock be soft like the limestones, then not only is the pebble rounded, but it is covered with a multitude of striæ crossing each other in all directions. These striated pebbles are of great importance in reference to the study of the ancient extension of glaciers; they are medals, the presence of which determines, almost with certainty, the former existence of a glacier, which has now disappeared. In fact, a glacier alone has the power to shape, wear, and striate these pebbles in the manner indicated. Water polishes and rounds them, but it never striates them. Nay, it effaces the

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* One of these wells, measured by MM. Dolfus, Otz, and myself, on the glacier of the Aar, was 58 metres deep. On the glacier of the Finsteraar, M. Desor sounded another, and found no bottom at 232 metres below the surface.

the striæ engraven by glaciers. This fact may be verified at the foot of those in the valley of Grindelwald. At 300 metres from the escarpment, the torrents which issue from it carry along only rounded pebbles, but they are smooth and completely deprived of striæ. I assured myself of this in the most positive manner. Indeed, M. Edward Collomb solved the question in an experimental manner. He took pebbles striated by the glaciers, and placed them along with sand and water in a horizontal cylinder, which was turned round only fifteen times in the minute. At the end of twenty hours, all the striæ had disappeared. Accordingly, we shall seek in vain for them on pebbles rolled along by the most violent torrents, or on banks where the flux and reflux of the sea washes continually, driving them to the shore, and again sweeping them back towards the deep.

After these details, we hope that the proofs which we adduce to demonstrate the ancient extension of existing glaciers, will be at least sufficiently intelligible. We have purposely omitted all that had not a direct application to the study of this great phenomenon. The method we adopt to prove this ancient extension, is at once the most simple and the most certain that can be followed in geology. We shall traverse the countries which surround the Alps, and inquire whether they present indubitable traces of the action of glaciers. If we everywhere find these traces as numerous and as evident, as in the neighbourhood of existing glaciers, we shall be inevitably led to admit, that formerly they descended into the plain, and filled the interval which separates the Alps from the Jura. The ancient extension of glaciers will be demonstrated, without it being in our power to give an account of the meteorological perturbations which accompanied them; for, in a study which dates only a few years back, we cannot flatter ourselves with having brought together a sufficient number of facts to enable us to ascend to the cause which produced this phenomenon. We may merely affirm, that this prodigious development of the seas of ice would be impossible in the existing climatic conditions, and that it necessarily supposes a notable depression in the temperature, and, consequently, a different climate from what now prevails in Europe.

5. *Of the ancient Extension of the Glaciers of Mont Blanc from Chamonix to Geneva.*

Before giving an idea of the extent of the antediluvian glaciers, I have thought that it would be of advantage to follow one of these glaciers throughout its whole length, from its origin to its terminal moraine. In this journey, we shall everywhere meet with marks which it has left in its passage, and we shall easily establish the identity of these marks with those found in the vicinity of actual glaciers. I choose, as an example, the glaciers of Mont Blanc, which formerly filled all the valley of the Arve, and extended from Chamonix to Geneva.

Let us transport ourselves to Montanvert, 850 metres above the village of Chamonix. The Mer de Glace is at our feet, it descends from the vast amphitheatres of the Jardin and the Aiguille du Geant. Without being bold mountaineers, we can pass the Ponts, cross the left lateral moraine, and advance to the promontory of the Angle. The whole surface of this promontory is polished and striated above as well as below the surface of the glaciers. Of this we may convince ourselves by looking between the ice and the wall of the granite. If we carry this examination further, we shall see that the rocks are polished and striated to a great height, and that the traces of the glaciers' action only cease at the foot of the high aiguilles which overlook it. Now, the striæ which the ice has engraved under our eyes being identical with those which are 300 metres above our heads, we are entitled to conclude that the thickness of the glacier, or its *puissance* (to use the language of the continental geologists), was formerly much greater than it is now. But if that thickness was greater, so also was its length; for a necessary relation exists between the three dimensions of a glacier. Thus, then, the terminal moraine, instead of being at the hamlet of Bois, 3 kilometres in advance of Chamonix, was at that time much more remote. We thus see that without quitting the surface of an existing glacier, we can be certain that its extent was formerly more considerable than in our day. But other proofs are not wanting.

Instead of stopping, like the glacier, at the foot of the mountain named Chapeau, the right lateral moraine is lengthened in the form of an immense mound, which bars the valley of Chamonix, and has the hamlet of Lavangi placed upon it. The Arve has worn out for itself a narrow passage between this mound and the northern declivity of the valley. In order to follow this route, we are obliged to enter upon this natural mole, and we are thus enabled to convince ourselves that it is composed of sand, pebbles, and large angular blocks confusedly heaped upon each other as in existing moraines. One of these blocks, placed on the ridge, is known by the name of *Pierre de Lisboli.* This mound is the ancient lateral moraine of the Mer de Glace; but the forest which covers it proves, that, for a long while, the surface of the glacier has sunk to the level where we now see it. Already, Saussure* had detected the existence of this ancient moraine, which appears with a distinctness of evidence which the most prejudiced mind cannot deny. It extends itself by ascending the valley as far as the hamlet of Iles, at 2 kilometres from the village of Argentière. The Arve, checked in its course by the moraine of Lavangi, formerly formed a lake, the successive levels of which are still indicated by the horizontal terraces which border the course of the torrent.

From the top of this lateral moraine, an attentive observer may perceive in the valley the ancient terminal moraine of the Mer de Glace, at the period of its smallest extension. The form of this moraine is characteristic; it is that of an arc whose concavity is turned up the valley. The village of Chamonix is partly built upon the moraine, and with the erratic blocks which compose it. The small mount situate on the left bank of the Arve, in front of the Union hotel, is one of its most salient points. In 1845, I could study the interior structure of this small mount while they were digging the foundations of the new hotel, erected in front of the one I have named, and I found that it was identical with that of the actual moraines.

But, it will be said, where is the proof that the erratic blocks of the moraine of Chamonix have been deposited there by the Mer de Glace? Is it not more natural to suppose that they have descended from the Brevent, whose eboulements continually threaten the village, and form the large delta, the eastern angle of which it occupies? The answer is easy. The Brevent is a gneiss mountain, and almost all the blocks of the moraine are of protogine, a species of characteristic granite which constitutes the mass of Mont Blanc, and of the sharp peaks with which it is surrounded.

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* Voyages dans les Alps, § 623.

Continue to descend along the valley. After crossing the Arve on a wooden bridge, you arrive at the hamlet of Montcuar, which is surrounded on every side by immense blocks of protogine. The ground, from being level, becomes unequal, and the route passes over numerous low mounds. You are on a new terminal moraine, corresponding to a greater extension of the Mer de Glace, and the glacier of the Bossons united; it is that of Montcuar, the breadth of which, on the banks of the Arve, is about 400 metres. This moraine terminates a little beyond the torrent which issues from the glacier of Taconnay. The blocks composing it are truly gigantic. All strangers remark those found in the small wood which runs along the side of the torrent. One of these blocks, named *Pierre Belle*, is not less than 24 metres 7 decimetres long, by 9 metres in breadth, and at least 12 in height. It is not a stone, but a true hill, rising above all the trees which surround it. If he retain some doubts as to the nature of the agent which transported these blocks, the observer who is not afraid of difficult paths, has only to ascend to the heights which overlook the right bank of the Arve. In the rude pathway which leads to the hamlet of Merlet, he will find, between 336 and 350 metres above the valley, moutonned rocks, that is to say, rocks rounded and polished like those we meet with under existing glaciers.

After traversing the moraine of Montcuar, the traveller advances along a ground formed of rolled pebbles, conveyed by torrents, whose dried up beds he may yet perceive; but, if he cast his eye on the right bank of the Arve, he perceives in the distance erratic blocks and large polished surfaces almost in a vertical position. He then finds himself near the village of Ouches, the last in the valley of Chamonix. It is there that the glacier has left the most varied and evident indications of its passage. The enormous pressure that must have been exerted to force the entrance of the narrow gorge of Montées, the change in the direction of the valley, all contribute to produce those phenomena which we observe at the foot of promontories, or near contractions which narrowed the bed of existing glaciers.

In front of the village of Ouches, on the right bank of the Arve, three hillocks are to be seen of a characteristic form; they are rounded in front, and precipitous behind. We at once perceive, that the force which has worn away the inclined strata of argillaceous steaschist which composes them, came from the upper part of the valley, and spared the face turned towards the lower part of it. Hence this rounded form in front, terminating abruptly in an escarpment turned in the opposite direction. Let us examine these hills nearer at hand; everywhere, on the summit and sides, we find rectilinear grooves,—those fine striæ running in the direction of the valley, and which could be traced only by glaciers; and, to complete the demonstration, numerous blocks of protogine, often enormous, with sharp angles and trenchant ridges, resting on polished and striated surfaces. Even to the height of 593 metres, the whole mountain of Coupeau, above the right bank of the Arve, is covered with moutonned rocks, which disappear, so to speak, under innumerable erratic blocks. The striæ which furrow these rocks are not horizontal; they could not be so, for this mountain forms a salient promontory in the valley, and the glacier having raised itself up against the obstacle which opposed its progress, has grooved the ascending striæ which run from the front backwards, like those we have mentioned on the glacier of the Aar, at the foot of the promontory which bears M. Agassiz's pavilion.

Thus the most convincing traces which a glacier can leave of its passage at the entrance into a defile, hills rounded in front and precipitous behind, moutonned rocks with grooves and rectilinear striæ, horizontal in the bottom of the valley, ascending on the promontory which contracted it, and a lateral moraine, composed of angular blocks suspended on the sides of mountains, are all found united at the entrance into the gorge of Montées.

There still are observers who ascribe all these appearances to the action of great currents of water. They think that these diluvian torrents have had the power to transport erratic blocks without blunting the angles, or effacing the ridges. They ascribe the rounded forms of the moutonned rocks, and the striæ which cover them, to the rapid passages of these blocks; they are not alarmed at the idea of admitting currents of 400 or 500 metres in depth, running during long periods of time, which presupposes masses of water really incalculable, and whose origin could not be explained. But the faith even of the most sturdy diluvialist, would, I think, be shaken, by comparing the marks of the ancient glacier which descended by the valley of Chamonix with the action of the Arve, which has scooped out a bed in the same ground which the glacier has modelled. On the one side moutonned rocks, furrowed with radiated grooves; in the interior, polished surfaces with fine striæ, always rectilinear, often ascending; enormous erratic blocks, with sharp angles and sharp edges, lying on the sides of mountains—such is the work of the glacier. On the other, erosions, sinuous and ramified canals, with smooth and uniform walls, always lying in the direction of the declivity; cylindrical cavities called *marmites de geans;* blocks of moderate size, rolled, rounded, the ridges and angles blunt, lying in the bottom of the valley—such are the effects of a torrent. They may be studied in the bed of the Arve by the side of the traces of the glacier. In the former case, it is a solid body which levels and engraves the rock; in the second, it is a liquid which incessantly attacks it, hollows and polishes it, but without causing a single ray.

On leaving the village of Ouches, the traveller crosses a small plain, and then enters the gorge of Montées, which unites the valley of Chamonix to that of Servoz. On the right the Arve roars at the bottom of a precipice, on the left a low and marshy place extends to the foot of Prairion. All the escarpments of the gorge of Montées, and all the rocks which rise in the valley are moutonned, strewed with large erratic blocks, and furrowed with rectilinear striæ, often many metres in length. Without leaving the great road, we may see one of those hills on the left bank of the Arve, after passing the bridge Pelissier; it is that on which the picturesque ruins of the tower of St Michael stands. Everywhere around these hills we find blocks of protogine covering polished and striated rocks. These blocks are often as it were suspended on the sides of the hill, in such positions that we are inevitably led to the conclusion that they have been transported by an agent which has deposited them gently and without shock in the place where they have remained *in equilibrio*, whereas an impetuous torrent would have hurried them along and precipitated them into the bottom of the valley.

What were the dimensions—the puissance—of the glacier when it made its way through the defile of the Montées! In order to answer this interesting question, I ascended the two banks of the Arve; on the right, above the rocks whose precipitous walls ran into the torrent, I found polished rocks and erratic blocks, at a height of 758 metres above the bridge Pelissier. On the left, not far from the Col de la Forclaz, the blocks rose to the height of 683 metres. These two points, situate opposite each other, are separated by a horizontal distance of at least 4 kilometres. The glacier was therefore a league broad at this point, and its medium dimensions were 720 metres (2215 French feet) at least; for, in measurements of this kind, one can never be certain that they have suspended the barometer precisely above the last polished rock, or near the last erratic block.*

Beyond the village of Servoz, the traces of the glacier of the Arve (it is by this name we shall henceforth distinguish it) disappear for some space. We pass over immense eboulements which have buried the moutonned rocks and the blocks of the moraine under a thick bed of debris. One of these eboulements, in 1751, was accompanied with such a terrible noise, and so dark a cloud of dust, that the authorities of the neighbouring town sent a courier to Turin to announce that a volcano had broken out among the Alps.

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* This thickness need not excite surprise, when we consider that that of the present glacier of the Aar near Abschwung is about 400 metres.

On the left bank of the Arve, the marks of the ancient glacier have not been concealed as on the right bank. If we follow the road which leads from the village of Chède to the baths of St Gervais, we again find blocks of protogine on the sides of the torrent, at the outlet of the narrow gorge through which it escapes to enter the valley of Sallenches. One of these blocks is surmounted by a pigeon-house, which attracts the attention of travellers from a distance.

The baths of St Gervaise are situate at the extremity of the valley of Montjoie, which runs along the western side of Mont Blanc, and cuts that of the Arve almost at a right angle. The torrent Bonnaut, which forms behind the baths a cascade celebrated among tourists, runs in the bottom of the valley. If the theory of the ancient extension of glaciers is not a vain hypothesis, the valley of Montjoie ought, like that of Chamonix, to send forth a glacier, and at its point of meeting with that of the Arve, we ought to recognise traces of the appearances which are observed in existing glaciers at the junction of two affluents. If these affluents are of equal force, they unite and advance in a parallel direction side by side; but, if they are of unequal size, the smaller is forced aside by the larger, and forms only a kind of wedge which penetrates more or less into the principal glacier. The union of the glaciers of Lauteraar and Finsteraar, is an example of a confluence of the former kind; the small glaciers of Thierberg, Silberberg, and Grünberg, which throw themselves into that of the Aar, shew us what takes place in the second instance. Compared with that of the Arve, the glacier of Bonnaut was but a weak tributary; however it has deposited its blocks at the entry of the valley of Montjoie, where, over a space of some kilometres, they alone cover the sides of the mountain between St Gervais and Combloux; but at the same time the glacier of Bonnaut, driving back the lateral moraine of the glacier of the Arve to the middle of the valley, has forced the blocks of protogine to recede from its edge. Accordingly, when the glacier of the Arve melted, these blocks, instead of remaining suspended on the sides of the valley of Sallenches, were deposited in the bottom, and we now find them lying around the gorge occupied by the baths of St Gervaise. We can even notice before the bath-house itself inclined beds of rolled pebbles, mingled with angular blocks, certain proofs of the ancient existence of a small glacial lake like that of Tacul, which is found in the angle formed by the junction of the glaciers of Geant and Lechaud, the principal affluents of the Mer de Glace of Chamonix.

At the distance of a few kilometres, the erratic blocks deposited by the glacier of Bonnaut are replaced by those of the lateral moraine of the glacier of the Arve, which reappears on the sides of the mountain, and prevails without interruption from the village of Combloux to the small town of Sallenches. It is to the learned Bishop of Annecy, M. Rendu, that we owe the discovery of this moraine. He had remarked with surprise, that the continuity of the cultivated fields, which, from the bottom of the valley, rise to a great height, was interrupted by a zone of forests. Entering within the shade of these dark pines, he immediately perceived the cause of this singularity. In this zone, the ground disappeared under an accumulation of erratic blocks, piled one above another, and rising to the height of the trees. Everywhere masses of protogine were seen, measuring from 10 to 20 metres in every direction. The ridges of these masses were as sharp, and the angles as acute, as in the moment when they were detached from the peaks of Mont Blanc. Not only have the trees shot forth between the blocks, but they have taken possession of the blocks themselves, and often a beautiful tuft of pines and birch vegetates, like a suspended forest on a foundation of granite. The traveller has as much difficulty in forcing a passage through this labyrinth, as if he had gone astray among the moraines of the Mer de Glace at Chamonix. Wherever the rivulets have cut up the ground, he notices this mixture of sand, pebbles, and angular blocks heaped confusedly, which characterizes the deposits made by glaciers. It is only at the depth of several metres that he finds the schistose strata of the mountain. The most gigantic blocks of the moraine of Combloux are found at the border of the wood below the village of that name; another, situate near the hamlet of Caches, at a little distance from Sallenches, is celebrated in the country under the name of *Pierre à Mabert.*

The great accumulation of blocks which renders the moraine of Combloux one of the most remarkable among the Alps, is easily explained, if we consider that at this point the buttress of the valley is exactly in front of the gorge of Servoz, by which the glacier of the Arve issued into the plain of Sallenches. This moraine was, therefore, at the same time both a lateral and frontal one, like that of the present glacier of Lauteraar, near Baerenritz. The imagination can scarcely compute the space of time during which the glacier deposited in this place the blocks severed from the Aiguilles, which surround Mont Blanc. Some have penetrated, along with them, the glacier of Bonnaut, in the high valley of Megève, which opens between St Gervais and Combloux; but they have not surpassed much the division of the waters of the Arve and Isère. The valley of the Megève not terminating in a cirque crowned with high mountains, it will be understood, that it does not give rise to a glacier, like the valley of Montjoie; but, as it opens on one side into the valley of the Arve, and on the other into that of the Isère, it is probable that two branches of glaciers of the same name met at the place where the burgh of Megève is now situate; for beyond that, towards the Isère, we find none of those blocks of protogine which characterize the glaciers of Mont Blanc.

Continuing to descend the course of the Arve, we enter the valley of Maglan; and we may satisfy ourselves that the moraine of Combloux does not terminate at Sallenches. Innumerable blocks of protogine cover all the declivities above the left bank of the river. In the defile of Cluses, many of these blocks are visible from the great road, and I have followed them to a height of 286 metres; which is certainly not the extreme limit of the moraine. These erratic blocks are wholly wanting on the right bank, throughout the whole valley of Maglan. Whence arises this difference? Why do we find thousands of blocks of protogine on the left bank of the Arve, and not one on the right? From Servoz as far as St Martin, in front of Sallenches, we may believe that these blocks are buried in the eboulements of the mountain of Fis, and the Aiguille of Varens; but above the cascade of Nant d'Arpenaz and the village of Maglan, the mountain presents uncovered terraces. M. Rendu has already solved this difficulty. He observes, that, at the height of Servoz, a powerful glacier coming from Buet, must have debouched on that of the Arve, by the Col d'Anterne. This considerable affluent, moving in a parallel direction to the glacier of the Arve, and forming its right side, carried along with it no blocks of protogine; its moraine was calcareous, like the mountains which overlook it. Now, the buttresses of the valley of Maglan being of the same nature, this moraine is confounded with the rocks that have fallen from the mountains. Nothing, in fact, is more difficult than to distinguish erratic blocks, when they have the same aspect, and are of the same mineralogical composition, as the rock on which they rest. On the other hand, these fragments of limestone, slate, and sandstone, have not resisted the influence of atmospheric agents like protogine, and have been in a great measure destroyed.

We perceive, that the theory of the ancient extension of glaciers explain very well the separation of the blocks of protogine, and of the calcareous moraine. The supposition of a diluvian is inadequate to resolve this difficulty. How, indeed, can we comprehend how an impetuous torrent, which would have carried along with it in confusion fragments of limestone and blocks of granite, would have deposited the one on its left bank, and the other on its right, without ever mingling them with each other? This supposition is inadmissible, and proves the insufficiency of the diluvian hypothesis.

The long lateral moraine which extends from Cluzes to Bonneville forms an uninterrupted zone along the whole right side of the valley. The last blocks of this moraine are often 640 metres above the Arve; witness those observed in the neighbourhood of the church of Mont Saxonex, whose elevated position and picturesque appearance attracts the eyes of the traveller from afar. The whole plain comprised between Bonneville and the mountain Saleve, is strewed with erratic blocks. These blocks, however, are wholly wanting on a band of 17 kilometres in length, and of variable breadth, which extends from the entry of the valley of Bornaud as far as Nangy, a village situate on the road from Bonneville to Geneva. This long band, known under the name of *Rocailles*, is almost wholly uncultivated, and its sterility contrasts strongly with the vigorous vegetation of the surrounding plain. The little town of Roche, the villages of St Laurent and Cornier, are built upon the Rocailles, while those of Pers, St Romaine, and Nangy, are placed on its edges. By penetrating to the middle of these rocks, many of which are 30 or 40 metres in height, near the imposing ruins of the Chateaux de la Roche, Chatelet, &c., and the towers of St Laurent and Bellecombe, the geologist finds himself all at once in a calcareous country. The mineralogical nature of the rocks which surround him, the white mud which covers the road, all confirm this idea. The botanist immediately recognizes the plants peculiar to limestone mountains, the box, cyclamen, and (*dompte-venin*) asclepias vincetoxicum; but these appearances are deceitful; wherever the torrents have ploughed up the ground, we see beds of mollase on which calcareous masses rest. The fossil shells which they contain complete the demonstration that those masses are not in their place, but that they have been formerly separated from the elevated parts of the mountain of Bornaud, and transported to the plain. We at last come to the conviction that the Rocailles are a great calcareous moraine, issuing from the valley of Bornaud, at the period when a glacier debouched on this valley, in order to unite itself to that of the Arve. At many points, we may see the granitic moraine and the calcareous one touching each other without becoming confounded; at the entry, for example, of the town of Roche on the side of Bonneville, and near the bridge Bellecombe, below the village of Nangy. At a kilometre above the village, all travellers remark two precipitous rocks rising near the road. The one supports a building, the Château de Pierre, the other a tuft of pines, having a most picturesque effect. These two rocks are the last blocks of the calcareous moraine of Bornaud, pushed forward, in former times, by the glacier as far as the right bank of the Arve.

Beyond Nangy, the plain comprised between the southern flank of Voirons and the eastern declivity of the Salèves mountains is strewed with blocks of protogine, accumulated chiefly on the plateau of Bornes, situate behind these mountains; but it is on the eastern side of the two Salèves that we must look for the terminal moraine of the glacier of the Arve. Notwithstanding an active clearing which has been going on for many years, the rounded shoulder of these two mountains is everywhere covered with these blocks. A great number of them have penetrated into the gorge of Monetier, others have remained suspended on the top of the precipice which looks towards Geneva, or have been precipitated into the plain the centre of which is occupied by that town. Near the village of Mornex, situate on the eastern slope of the smaller Salève, we likewise find polished rocks, and considerable collections of sand, gravel, and striated pebbles. Thus all the proofs of the ancient existence of a glacier are united on the eastern declivity of the Salèves, as visible, as indisputable, as in the valley of Chamonix, the cradle of the gigantic glacier whose traces we have followed. For it, the Salèves presented no insuperable barrier; it passed their summits, turned round their extremities, and threw its last blocks on Mount Sion, an elevation situated on the south of Geneva, and the point where the waters are divided, and flow to Lake Leman or to that of Annecy. Blocks of protogine occupy the most elevated parts of Mount Sion, and the last group crowns the summit of a hill rising above the village of Vers, near the road from Geneva to Chambery.

On two sides of Mount Sion, the geologist finds erratic blocks of a very varied nature; and, recollecting the mountains where these rocks form considerable masses, he becomes convinced that he is at the point where three great antediluvian glaciers met,—that of the Rhone, which filled the whole of the Leman basin; that of the Isère, which debouched by the lakes of Annecy and Bourget; and that of the Arve, which, inserting itself between them like a sharp wedge, came to its termination near the village of Vers. The humble Mount Sion was, as is said by M. Arnold Guyot, to whom we owe this beautiful discovery, the point of convergence for these powerful glaciers which have so essentially modified the surface of the plain comprised between the Alps and the Jura. We shall not follow them in all their windings, for all of them present us with particulars analogous to those of the glacier of the Arve. Let us merely trace the great features of the limits of the ancient extension of these glaciers.

The glacier of the Rhone had its origin in all the lateral valleys which intersect the two parallel chains of the Valais, where the most elevated mountains in Switzerland are found, Mont Rosa, Mont Cervin, the Jungfrau, Velan, &c. This glacier filled the Valais, and spread itself in the plain comprised between the Alps and the Jura, from Fort l'Ecluse as far as the environs of Aarau. It was the principal glacier of Switzerland; it is it which has conveyed the innumerable blocks which cover the Jura to a height of 1040 metres above the sea. The other glaciers were only feeble affluents of the glacier of the Rhone, incapable of making it deviate from its direction. Thus, when the glacier of the Arve met it on the ridge of the Salèves or on the sides of the Voirons, we perceive, by the disposition of the moraines, that the glacier of the Rhone continued its progress, while that of the Arve was suddenly arrested. In like manner, a rapid river repels the feeble rivulet which brings the tribute of its waters.

Other secondary glaciers occupied the principal valleys of Switzerland. Such were the glacier of the Aar, whose last moraines crown the hills in the neighbourhood of Berne; that of the Reuss, which has covered the shores of the Lake of the Four Cantons with blocks severed from the peaks of St Gothard. That of De la Linth stops at the extremity of the Lake of Zurich, and the town is built upon its terminal moraine. Finally, that of the Rhine, less studied than the others, occupied all the basin of the Lake of Constance, and extended to the adjacent parts of Germany.

It appears, therefore, that during the period of cold which preceded the appearance of man upon the earth, Switzerland was a vast sea of ice, one of whose roots were buried in the high valleys of the Alps, while the terminal escarpment rested on the Jura. In like manner, on the southern declivity of the chain, the glaciers descended into the plains of Piedmont and Lombardy. Those of the southern side of Mont Blanc united to form the glacier of the Val d'Aoste. Its terminal moraine rose like a gigantic mound in the neighbourhood of the town of Yvrée; it is the *Serra* of Piedmont. The greater part of the lakes of Upper Italy owe their existence to the frontal moraines of these great glaciers; by obstructing the course of the rivers, they have forced them to extend themselves in the form of liquid sheets. Among the most evident moraines, I may mention the three concentric arcs which bound the extremity of the Lago Maggiore, near Sesto-Calende; those of the Lake Garda are not less distinctly characterised, in the environs of Desenzano and Peschiera.

6. *Of the Climate of the Glacial Epoch.*

When the imagination conceives all the countries which surround the Alps buried under the ice to the distance of many leagues, it trembles, so to speak, at the idea of the dreadful cold which must result from this prodigious development of the Alpine glaciers, It would seem that the climate of Siberia presents nothing sufficiently rigorous to explain the permanent existence of this mantle of ice extended over countries which now enjoy a temperate climate. It is easy to shew that these notions are exaggerated.

In fact, what we have said respecting the transformation of snow into ice by repeated meltings and congelations, must lead us to understand that there could not be glaciers with a climate of extreme severity, such as that in the north of Siberia. Spitzbergen, which realises in the highest degree the conception of a country enveloped in glaciers, since they descend in all directions even to the sea, has a mean temperature of 8 centigrade degrees below zero; that of summer, is 2°·4 above it. Iceland, where the glaciers rest on the shores of the sea, but never pass beyond them, like those of Spitzbergen, presents, at different points, a mean temperature comprised between zero and +4°. We could form an idea, by means of a very simple calculation, of the climate which would bring the glaciers of Mont Blanc to the margin of the

Lake of Geneva. The mean temperature of that town is 9·°56. On the surrounding mountains, the limit of perpetual snow, as we have seen, is found to be 2700 metres above the sea. The great glaciers of the valley of Chamonix descend to 1550 metres below that line. This settled, suppose that the mean temperature of Geneva was lowered 4 degrees only, and became 5°·56; the decrease of the temperature with the height being one degree for 188 metres, the limit of perpetual snow would descend to 750 metres, and would be more than 1955 metres above the sea. It will be readily allowed that the glaciers of Chamonix would descend below this new limit in a proportion at least equal to that which exists between the actual limit and their lower extremity. Now, at present the foot of these glaciers is at 1150 metres above the ocean; with a climate 4 degrees colder, it would be 750 metres lower, that is to say, it would come to the level of the Swiss plain. Thus, then, the lowering of the line of eternal snow would suffice to make the glacier of the Arve descend to the neighbourhood of Geneva. But we must not forget that a glacier descends so much lower in proportion to the extent of the amphitheatre from which it proceeds; now, the glaciers, having, as a source of supply all the valleys and all the gorges elevated above 1950 metres, would descend, from that cause alone, much lower than before. Accordingly, the united action of these two causes, the lowering of the line of eternal snow, and the enlargement of the amphitheatres, causes, each of which, taken by itself, would be sufficient to explain the ancient extension of glaciers, enable us readily to comprehend how that of the Arve could formerly advance to the environs of Geneva. Let it not be forgotten, that this extension has been the work of a long series of ages, the number of which is, so to speak, declared to us by those millions of blocks which the glacier has slowly and successively carried from the foot of Mont Blanc to the margin of the Leman lake.

The climate which has favoured this prodigious development of glaciers has nothing of which we cannot form a very exact idea; it is the climate of Upsal, Stockholm, Christiania, and the northern part of America in the state of New York. Geologists who do not hesitate to raise the mean temperature of cold or temperate zones from 10 to 20 degrees in order to explain the presence, in the bowels of the earth, of tropical ferns or the animals of warm countries, would with very bad grace, in my opinion, shew alarm at this alteration of the mean annual temperature, although the proposed change takes place in an opposite sense, and the thermometer descends instead of ascends. If we admit that the climate of one portion of the globe may have undergone change, it is as legitimate to suppose that it has become colder, as to maintain that it has become warmer; and to diminish by 4 degrees the mean temperature of a country, in order to explain one of the greatest revolutions of the globe, is assuredly one of the least hazardous hypotheses which a geologist may be permitted to form.

To discuss the causes which have produced this sinking of temperature,—to indicate the geological or meteorological changes which have brought on this long period of cold, appears to me an attempt altogether premature. It would be necessary, before every thing else, to draw up a map of the extension of glaciers; now, this can scarcely be said to have been attempted even for the Alps, the Vosges, and the mountains of Scotland. Ancient moraines exist in the Pyrenees, Altai, Caucasus, and Atlas; but no one has yet examined the topography of the glaciers which have carried these before them. Sweden, Norway, Denmark, Finland, and North America, were covered with great fields of ice. What of a positive nature can, therefore, be said respecting a phenomenon with whose extent we are yet unacquainted? Let us not imitate our predecessors, whose brilliant imagination rested the boldest generalisations on the frail basis of a few insulated and incomplete facts. All these hasty works are destined to perish. Science has revealed to us a new epoch in the history of our planet; a vast field opens before natural philosophers, astronomers, and naturalists. Let us not fear to turn a searching look into the depths of this remote distance, the traces of which have been preserved by the surface of the earth, but let us reject hypotheses which outstrip facts, and which a fact the most trifling in appearance overturns without mercy. By the side of the diluvian period, we see the glacial period take its place; let us hail the appearance of this latter, for it has been revealed to us by the attentive study of well observed facts, and not by vain speculations of the fancy. Let us not renew the idle quarrels of the Neptunians and Vulcanists; impartial posterity has decided between them. They were equally wrong as impassioned partisans of an exclusive idea; each of them had reason on their side by the facts and observations they brought forward in support of their absolute theories. All existing geologists are at once Vulcanists and Neptunians; science has taken the part of water and fire. It will be the same with glaciers and currents. Both of them have played their part in times past, and it is the same at the present time. The phenomena have remained the same; but, instead of those gigantic manifestations, which is the character of the geological epochs anterior to our own, they are included within the limits of action imposed upon them by the equilibrium of the period of repose which the appearance of man has introduced to the earth.—*From Revue de Deux Mondes*, t. xvii. 1st March 1847.

On the Colour of the Ice of Glaciers, and that of the Waters which flow from them. By M. Ch. Martins.

At the meeting of the 15th of March last, the Academy received a communication from M. Durocher, entitled *Etudes sur les Glaciers du Nord et du centre de l'Europe.* This note contains some assertions which it appears to me of importance to rectify. The author has stated (p. 144), "that the interposition of water between the pores and fissures of granulated ice contributes largely to develop in them that beautiful blue colour which is so much admired." It is long, indeed, since M. Agassiz has proved that blue ice contains more water than white ice; but the blue colour appears to me to be owing rather to the absence of air than the presence of water, for the white ice is infiltrated with water like the blue, but the white ice is wholly filled with spherical bubbles of air. In the blue ice, these are replaced by water of infiltration. M. Celestin Nicollet has proved this by direct measurement, shewing that 500 grammes of blue ice contain only 0·5 cubic centimetres of air, while the same weight of white ice contains 7·5 centimetre cubes; accordingly, when we look at a fragment of ice half blue and white, we see that the white part is filled with bubbles of air, while the blue part is almost wholly without them. M. Dollfus, again, has shewn that the specific weig of white ice is 871; that of blue ice 909; that of water being 100 a result which confirms the preceding.

M. Durocher then affirms, "that the waters which flow from fiel of snow and glaciers have a very decided sky-blue tint. When," adds, "the detritus mingled with water are grey, they produce no oth effect than to make the blue tint paler, and make it pass into da blue." This assertion appears to me to be very rash, particular when it is applied equally to the waters furnished by melted sno and those which come from the melting of glaciers. The former ar more or less pure, but the colour varied, without, however, presentin the limpidity of that of springs. Thus, the little lake of Lioson, i the Canton de Vaud, fed by the snows of Tête-de-Moine, is of th most beautiful azure. The Bachalp-See, situate 2·275 metres abov the sea, which receives the waters flowing from the snows of the Faulhorn, is of a yellowish-green.

Let us bestow a few words on the consideration of the waters which issue from glaciers. They are always charged with a considerable portion of mud, produced by the pulverisation of the fragments of rock which the glacier grinds down in its ceaseless progress. Thus, water taken from the surface of the Aar, at some metres from the glacier of the same name, contains, according to the experiments of M. Dollfus, 142 grammes of impalpable powder in a cubic metre of water. It is the same with the torrents which issue from glaciers. All of them roll along in a turbid, grey, milky, or dark stream, according to the nature of the pulverised rock. Hence the vulgar denominations of *torrents*, derived from the colour of their waters. The *black water*, which throws itself into the Rhone, near Martigny, in the Valais, comes from the glaciers of Trient and Buet. The *white Lutschine* derives its origin from the numerous glaciers which occupy the bottom of the valley of Lauterbrunn; and the *black Lutschine* comes from those of the valley of Grindelwald. Can it be true, as M. Durocher states (p. 445), that "the mud of glaciers has no other effect than to make the blue tint become paler, and pass into a muddy blue?" The Swiss geologist Ebel expressed the same idea thirty years ago: "the water of glaciers," he says,* "is of a whitish-blue, and the torrents which issue from them preserve this colour for many leagues, when other rivulets do not alter them by mixing with them." This, we perceive is the same opinion which M. Durocher has reproduced before the Academy. Nevertheless, I believe it to be incorrect. The waters which flow from glaciers have never appeared to me blue; this is also M. Agassiz's opinion, when he says (page 574 of his work on existing glaciers, which will be published speedily), "It is the mud which gives the water of glaciers *the milky tint which characterises them.*"

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* Manuel au Voyageur en Swisse. 3d Ed., French Trans., tome ii., p. 521. Article GLACIERS.

In order to throw some light on the question, let us now consider the colour of the water of glaciers in a state of repose, where it is accumulated in great masses in the lakes of Switzerland. The lake of Geneva, fed principally by the waters of the Rhone, which come from all the glaciers of the Valais, is, it is true, of a beautiful azure blue; but the lake of Brientz, which receives the waters of glaciers exclusively, is of a very deep *pistachio-green*. This tint is equally striking, when we view it from the summit of the Faulhorn, that is to say, 2120 metres above its level, or only from a few metres above its surface. M. Bravais and myself have observed that, for entire weeks, this tint varies only very little, according to the light. The circumstance which proves decidedly that these tints are independent of the glacier origin of the tributaries is, that the lake of Thun, receiving its waters from the lake of Brientz, with which it communicates across the isthmus of Interlaken, is of a blue colour, sometimes equalling that of the lake of Geneva. The small Gelmersee, fed by the glaciers of the Geemerhorn, is pistachio-green, like the lake of Brientz. I may mention, besides, the lake of Kloenthal, in the Canton of Glaris. "One can scarcely distinguish it," says M. Raoul-Rochette, in his *Lettres sur la Swisse*, "from the meadow surrounding it; for its waters are of the same colour as the adjoining grass." This lake is fed by the glaciers of Glaernisch and the melted snow of Pragel. The small lake near the hospice of Grimsel is of a sombre tint; others are more or less blue; and this variability of hue proves that the character assigned by Ebel and Durocher to the waters of glaciers is subject to such numerous exceptions, that it loses all scientific value.

In order to finish this demonstration, let us turn our attention to the colour of the waters of glaciers when they make their escape in the form of rivers, from the lakes in which they have deposited the impurities with which they were charged. Nothing can equal the azure of the waters of the Rhone when they issue from the lake of Geneva; they form a striking contrast with the grey and muddy waves of the Arve, which comes directly from the glaciers of Chamonix. The only example cited by M. Durocher is of the same kind; he refers to the Otta-Elv, a river of Guldbrandsdal, a valley of Southern Norway. This river traverses four lakes in succession before receiving the tributary of Lougen-Elv. It is not therefore surprising that its waters, purified in passing through these four basins, contrast in their blue colours, with the waters of the Lougen-Elv; this is exactly the case of the Arve and Rhone at their junction below Geneva. Both of them come from glaciers; but the Rhone, grey like the Arve when it enters the lake of Geneva, is blue when it leaves it, because it is freed from its impurities. The Arve continues turbid, because it passes through no lake in its course. The following is the last fact we shall mention, as affording a still stronger proof that ~~the blue colour is not inherent in~~ the water of glaciers: At the top of the valley of Kandersteg (canton of Berne), 1568 metres above the sea, is the small lake of Oeschinen; it is fed wholly by the waters which fall in numerous cascades from the glaciers of the Doldenhorn. This lake is of a deep green. Its outlet being subterranean, the waters pass through the ground as through a filter; they are limpid, transparent, shewing no trace of blue, of a sombre tint in deep places, and remind the spectator, in every point of view, of the pure and colourless waters where cresses grow, and the small rivers of Normandy inhabited by trouts. We perceive that, in this case, the waters furnished solely by glaciers, and after a very short transit, do not exhibit the blue tint either in a state of repose or motion.*

On the Colour of Water.

"I have," says Sir H. Davy, "often thought upon the subject, and I have made some observations and one experiment in relation to it. I will give you my opinion with pleasure, and, as far as I know, they have not been brought forward in any of the works on the properties of water, or on its consideration as a chemical element. The purest water with which we are acquainted, is undoubtedly that which falls from the atmosphere. Having touched air alone, it can contain nothing but what it gains from the atmosphere; and it is distilled without the chance of those impurities, which may exist in the vessels used in an artificial operation. We cannot well examine the water precipitated from the atmosphere, as rain, without collecting it in vessels, and all artificial contact gives more or less of contamination. But in snow, melted by the sunbeams, that has fallen on glaciers, themselves formed from frozen snow, water may be regarded as in its greatest state of purity. Congelation expels both salts and air from water, whether existing below, or formed in, the atmosphere; and in the high and uninhabited regions of glaciers, there can scarcely be any substances to contaminate. Removed from animal and vegetable life, they are even above the mineral kingdom; and though there are instances in which the rudest kind of vegetation (of the fungus or mucor kind) is even found upon snows, yet this is a rare occurrence, and red snow, which is occasioned by it, is an extraordinary and not a common phenomenon towards the pole, and on the highest mountains of the globe. Having examined the water from melted snows on glaciers in different parts of the Alps, and having always found it of the same quality, I shall consider it as pure water, and describe its characters. Its colour, when it has any depth, or when a mass of it is seen through, is bright blue; and according to its greater or less depth of substance, it has more or less of this colour. As its insipidity and its other physical qualities are not at this moment objects of your inquiry, I shall not dwell upon them. In general, in examining lakes and masses of water in high mountains, their colour is of the same bright azure; and Captain Parry states, that the water on the Polar ice has the like beautiful tint. When vegetables grow in lakes, the colour becomes nearer sea-green; and as the quantity of impregnation from their decay increases, greener, yellowish-green; and at length, when the vegetable extract is large in quantity, as in countries where peat is found, yellow, and even brown. To mention instances, the Lake of Geneva, fed from sources (particularly the higher Rhone) formed from melting snow, is blue, and the Rhone pours from it dyed of the deepest azure, and retains partially this colour till it is joined by the Soane, which gives to it a greener hue. The lake of Morat, on the contrary, which is fed from a lower country, and from less pure sources, is grass-green. And there is an illustrative instance in some small lakes fed from the same source, in the road from Inspruck to Stutgard, which I observed in 1815 (as well as I recollect), between Nazareit and Reiti. The highest lake fed by melted snows in March, when I saw it, was bright blue. It discharged itself by a small stream into another, into which a number of large pines had been blown by a winter storm, or fallen from some other cause; in this lake its colour was blue-green. In a third lake, in which there were not only pines and their branches, but likewise other decaying vegetable matter, it had a tint of faded grass-green, and these changes had occurred in a space not much more than a mile in length. These observations I made in 1815. On returning to the same spot twelve years after, in August and September, I found the character of the lakes entirely changed. The fine wood washed into the second lake had disappeared; a large quantity of stones and gravel, washed down by torrents, or detached by an avalanche, supplied their place; there was no perceptible difference of tint in the two upper lakes, but the lower one, where there was still some vegetable matter, seemed to possess a greener hue. The same principle will apply to the Scotch and Irish rivers, which, when they rise or issue from pure rocky sources, are blue, or bluish-green; and when fed from peat-bogs or alluvial countries, yellow, or amber-coloured, or brown, even after they have deposited a part of their impurities in great lakes. Sometimes, though rarely, mineral impregnations give colour to water, small streams are sometimes green or yellow from ferruginous depositions. Calcareous matters seldom affect their colour, but often their transparency when deposited, as is the case with the Velino at Terni, and the Anio at Tivoli; but I doubt if pure saline matters, which are in themselves white, ever change the tint of water.—(*Salmonia, by Sir Humphrey Davy*, p. 198.)

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* Comptes Rendus, No. 13, p. 545.

John-Crow Vulture,[*] (*Turkey-buzzard,* Wilson; *Cathartes aura, Vulture aura,* Linn; *Cathartes aura,* Illiger.) Ann. pl. 151.[†]

The history of this species has been so ably written by Wilson and Audubon, that I shall do little more than touch on one or two disputed points in its economy. An excellent memoir of this vulture, communicated to me by my valued friend, Richard Hill, Esquire, of Spanish Town, affords some interesting particulars:—

" Notwithstanding it forms so common a feature in our landscapes, being seen every day and everywhere, on the mountain as well as on the plain, in the city as well as in the country, the Aura is not common to the West Indies. It exists in Cuba and Trinidad, but is unknown in Hayti, and in all the intermediate islands of the Caribbean chain. We are no doubt indebted for it to an accidental colony, blown over to us from Cuba, and Cuba itself owes it to some stray visitants from the neighbouring continent of Florida. Some similar fortuity imparted to us, in common with Cuba, from America, its naturalized hive-bee, which is said to have been at comparatively a late period an introduction into St Domingo.

" Those who ascribe the power which the vulture possesses of discerning from a distance its carrion food, to the sense of seeing or to the sense of smelling *exclusively*, appear to me to be both in error. It is the two senses, exerted sometimes singly, but generally unitedly, which give the facility which it possesses of tracing its appropriate food from far distances * * * * I shall relate one or two occurrences, which seem to me to be instances in which the sense of seeing and the sense of smelling were, sometimes separately, and sometimes unitedly, exerted by the vulture in its quest for food.

" A poor German immigrant who lived alone in a detached cottage in this town, rose from his bed after a two days confinement by fever, to purchase in the market some fresh meat for a little soup. Before he could do more than prepare the several ingredients of herbs and roots, and put his meat in water for the preparation of his pottage, the paroxysm of fever had returned, and he laid himself on his bed exhausted. Two days elapsed in this state of helplessness and inanition, by which time the mass of meat and pot-herbs had putrefied. The stench becoming very perceptible in the neighbourhood, vulture after vulture, as they sailed past, were observed always to descend to the cottage of the German, and to sweep round, as if they had tracked some putrid carcase, but failed to find exactly where it was. This led the neighbours to apprehend that the poor man lay dead in his cottage, as no one had seen him for the two days last past. His door was broken open: he was found in a state of helpless feebleness, but the room was most insufferably offensive from something putrefying, which could not immediately be found, for the fever having deprived the German of his wits, he had no recollection of his uncooked mess of meat and herbs. No one imagining that the kitchen pot could contain anything offensive, search was made everywhere but in the right place; at last the pot-lid was lifted, and the cause of the unsupportable stench discovered in the corrupted soup-meat.

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[*] Length $25\frac{1}{2}$ inches, expanse 66, tail $9\frac{1}{2}$, wing from flexure $20\frac{1}{4}$, rictus $2\frac{2}{10}$, arms 3, middle toe $2\frac{1}{10}$, claw $\frac{9}{10}$.

[†] The above is extracted from an interesting volume on " The Birds of Jamaica," by Philip Henry Gorse and Richard Hill, Esquires, of Spanish Town, *Jamaica,* just published by Mr Van Voorst, London.

"Here we have the sense of smelling directing the vultures, without any assistance from the sense of sight, and discovering unerringly the locality of the putrid animal matter, when even the neighbours were at fault in their patient search.

"Some few days succeeding this occurrence, after a night and morning of heavy rain, in which our streets had been inundated to the depth of a foot, and flood after flood had been sweeping to the river the drainage of the whole town, a piece of recent offal had been brought down from some of the yards where an animal had been slaughtered, and lodged in the street. A vulture beating about in search of food, dashed in a slanting direction from a considerable height, and just resting, without closing his wings, snatched up the fresh *piece of flesh, and* carried it off.

"Here was the sense of sight, unassisted by that of smelling, for the meat was too recent to communicate any taint to the morning air, and the vulture stooped to it from a very far distance.

"On another occasion, very near to the time when these facts attracted my notice, a dead rat had been thrown out, early in the morning into the street, having been caught in the previous night. Two vultures sailing over head in quest of a morning meal, descended at the same time, stooping to the dead rat, the one from the south the other from the north, and both seized the object of attraction at the same moment.

"Here, again, was the vision, unaided by the sensitiveness of the nostrils, directing *two* birds, with the same appetite, at the same moment, to the same object.

"For the next example, I am indebted to the records of a police-court. A clerk in the engineer department at Uppark Camp, brought before the magistrates of St Andrews, on the 20th January 1840, a man who had been beset in the night by the dogs of the barracks. The poultry-yard had been repeatedly robbed; and this person was supposed to have been prowling after the roost-fowls at the time the dogs rose upon him." This case had been heard, and the man committed to the House of Correction, when a complaint was presented against another man, whom Major G., also of the camp, had detected under similar circumstances, and lodged in the guard-house. Two days after his detection, "the Major observed some carrion-vultures hovering about a spot in the fields, and on sending to see what was the matter, a Kilmarnock cap, containing a dead fowl and some eggs, tied up in a pair of old trousers, was found very near to the spot where the prisoner was caught. This discovery by the aid of the vultures confirming the suspicion against the prisoner, he was condemned.

"The last instance that I shall relate is one in which the senses of hearing, seeing, and smelling, were all exercised, but not under the influence of the usual appetite for carrion food, but where the object was a living, though wounded animal.

"A person in the neighbourhood of the town having his pastures much trespassed on by vagrant hogs, resorted to his gun to rid himself of the annoyance. A pig which had been mortally wounded, and had run squeaking and trailing his blood through the grass, had not gone far before it fell in the agonies of death; at the moment the animal was perceived to be unable to rise, three vultures at the same instant descended upon it, attracted no doubt by the cries of the dying pig, and by the scent of its reeking blood, and while it was yet struggling for life, began to tear open its wounds and devour it.

"These several instances, I think, abundantly shew that all the senses are put in requisition by the John-Crow vulture in its quest for food."

From the facts thus presented by Mr Hill, we gather also, that the common opinion is erroneous, which attributes to the vulture a confinement of appetite to flesh in a state of decomposition. Flesh is his food, and that he does not pounce upon living prey like the falcons, is because his structure is not adapted for predatory warfare, and not because he refuses recent, and even living flesh, when in his power. If the John-Crow vulture discovers a weakling new-born pig apart from the rest, he will descend, and seizing it with his beak will endeavour to drag it away: its cries of course bring the mother, but before she can come, the vulture gives it a severe nip across the back, which soon ensures the pig for his own maw. If a large hog be lying in a sick condition beneath a tree, the vulture will not hesitate to pick out its eyes, having first muted upon the body, that it may discover whether the animal be able to rise; the contact of the hot fæces arousing the hog if he be not too far gone. Cattle also he will attack under similar circumstances. One of my servants once saw a living dog partly devoured by one. The dogs of the negroes, half-starved at home, "bony, and gaunt, and grim," if they discover carrion, will gorge themselves until they can hardly stir, when they lie down and sleep with death-like intensity. A large dog thus gorged was sleeping under a tree, when a John-Crow descended upon him, perhaps attracted by the smell of the carrion which the dog had been devouring, and began tearing the muscles of the thigh; it actually laid open a considerable space before the poor animal was aroused by the pain, and started up with a howl of agony. The wound was dressed, but the dog soon died.—*The Birds of Jamaica, by Philip Henry Gorse*, p. 1.

Series of Researches on the Geology of that part of the Alps lying between the Valais and Oisans. By M. FOURNET.

In 1841, M. Fournet published in the *Annales de la Société d'Agriculture de Lyon*, vol. iv., two memoirs on the Geology of that part of the Alps comprised between the Valais and Oisans. It is a grand and interesting subject of study; but M. Fournet had scarcely entered, so to speak, into the matter, his attention being hitherto directed to general considerations. He indicates four systems of elevation (*soulevement*) which have crossed each other in the Alps; 1*st*, The system of the Rhine; 2*d*, System of Mont Viso, running from NW., to SE.; 3*d*, System of Western Alps, lying from NNE. to SSW.; 4*th*, System of the Valais, directed from ENE. to WSW. The author then passed to a description of the rocks of plutonic origin, which are so numerous and varied in the Alps. He divides them into four groups; 1*st*, Micaceous rocks; 2*d*, Serpentine-talc, or Magnesian rocks; 3*d*, Porphyritic rocks; 4*th*, Pyroxenic rocks.

In this description of the rocks, he has entered upon interesting researches. We would mention more particularly those on the origin of agates (*agathes*) and zeolites, which he went to study at Oberstein in the Palatinate, in order to understand better the glandular rocks of the Alps.

M. Fournet has published a continuation of these two memoirs. He treats of a higher subject, namely, the study of the modifications produced on sedimentary rocks by the vicinity of rocks of igneous origin. He considers not only the metamorphisms which have taken place on a small scale; but also the modifications undergone by the whole sedimentary rocks of the Alps. For a long time the necessity has been felt of treating certain branches of Geology chemically, particularly the metamorphism, and we are happy to find a work so carefully and accurately executed in this respect as that of M. Fournet.

De Saussure had likewise described a portion of the *section* from Martigny to St Maurice, and he did it with his usual admirable precision. He particularly attached himself to the study of petrosilex. But, in his time, the theory of metamorphism, at least such as it is now understood, had not been promulgated; consequently, M. Saussure's work and that of M. Fournet, regard the subject in two very different points of view.

The valley of the Rhone, between Martigny and St Maurice, in the Valais, intersects the prolongation of the axis of elevation of Mont Brevent, (valley of Chamonix); this axis is formed of rocks of crystallisation; it is parallel to the axis of Mont Blanc, and it has upraised sedimentary rocks belonging for the most part to the jurassic formation. All these rocks have been traversed by a great number of veins which are the principal cause of the metamorphic effects. These veins appear upon the escarpments, sometimes under the form of transverse bands, sometimes under that of large lenticular masses parallel to the stratification; they are found in all the rocks placed between the slates of La Bathia (near Martigny) and the ravine of Saint Barthelemy, below the Dent du Midi. They are composed of quartz, pure felspars, or associated with quartz, or rather with some crystals of amphibole (?), protogine, and rocks allied to serpentine. These rocks are frequently ill defined and confusedly crystallised.

The rocks which are thus traversed with veins are the following, going in the direction from Martigny to St Maurice, that is to say, from the superior to the inferior.

At Bathia, we see slates slightly micaceous, with small veins of calcareous spar and milky quartz, thin grey slates passing into unctuous slates, crossed by veins of quartz and a felspar rock. These latter, by their contact, determine the formation of the petrosilex or palaiopetres of Saussure.

Further down, near Trient, are various kinds of *gneiss*, associated with petrosilex, or even mingled with them; and amongst the *debris we find* saccharoidal limestone with ido-

crases; then come the conglomerates of Valorsine, with their micaceous paste and black slates. At the cascade of Pissevache, we again find petrosiliceous veins; at the cascades of Mieville, we perceive felspar and micaceous rocks. Finally, from Mieville to Evionnaz, the geological chart of France, indicates that the valley intersects the axis of the chain of Brevent. All this system is parallel to the Western Alps.

"If we take account," says the author, "of the result of metamorphic actions, we may group these products (these stratified and altered formations, in the following manner:—

"1*st*, The baking of schistose clay into argillaceous or clay slates.

"2*d*, The conversion of ordinary slates into hard unctuous slates, more or less modified in their colours.

"3*d*, Complete hardening or semi-fusion, which has caused the slates to pass into a petrosiliceous state.

"4*th*, Crystallisation into chlorite of the slaty fragments embedded in the veins of quartz.

"5*th*, Transformation of the clay slates into green slates, more or less felspathic, and very variable in the grain.

"6*th*, Change of the same rocks into felspathic micaceous slates, or into gneiss more or less porphyritic, in consequence of an imbibition and crystallisation of mica and felspar.

"7*th*, Lastly, crystallisation of black limestones into white limestones, saccharoidal, or saline, with or without idocrase."

He then proceeds to examine the different changes which the rocks have undergone. It then becomes indispensable to inquire into the nature of the rocks. Now, two kinds here present themselves to him as fundamental, namely, the limestone and clay state. M. Fournet first takes up the latter, the metamorphism of which appears to be more general, more complicated, than that of the limestone, on account of its variety of composition, and its exfoliability,—a circumstance which multiplies the points of contact. What is the composition of clay-slate? What is its constitution? Is it itself a metamorphic rock? These are some of the problems discussed.

According to various analyses, this rock is always formed of from 59 to 70 per cent. silica, from 10 to 23 per cent. of alumina, from 6 to 11 per cent. of oxide of iron, and from 2 to 6 per cent. of water; frequently other bases are added to this composition. "It will be observed from the first," says M. Fournet, "that their composition (that of the clay-slates) is homogeneous enough to entitle us to expect to find results sufficiently agreeing between the metamorphism of different localities, and thence also the character of generality which these phenomena present."

The clay-slates are formed, according to the analyses, of hydrosilicates, combined with silicates; and M. Fournet thinks that they owe their origin to clays, which have been converted into clay-slate by calorific effects. In order to prove this, he brings forward different modes of reasoning, one of the most remarkable of which is drawn from the state of the colouring matter of the rocks of the Alps.

This colouring matter is commonly bitumen, which is found decomposed into nearly pure carbon. Indeed, it appears, from numerous examples that, among the Alps, at the contact of the eruptive veins, the bitumen has been often converted into graphite. He has found plates of it in the saccharoidal limestone, associated with the idocrases of the Coupe de Martigny. This fact had already been announced by M. Elie de Beaumont, in his work on the Col du Chardonnet.

But beyond the point of immediate contact, to a distance of a mile or more from the plutonic masses, there may be, in certain cases, a more or less complete conversion of the bitumen into anthracite; and at a distance of several miles, the bitumen is not decomposed. These distances, it will be understood, may vary according to the conducting power of the rocks, the power of the eruptive mass, the number of veins, and, finally, the disposition of the beds. It will be observed, moreover, that the above measures have been taken horizontally, so that they give merely an approximation. However, the result is not less certain, that the whole thickness of the jurassic formation of the interior parts of the Alps, has felt the influence of a temperature capable of altering the bitumens.

We shall not follow our author in his examination of the properties of slates, such as their colour, their state of molecular aggregation, &c., with the view of finding in them the products of a more or less elevated temperature. We may merely mention, that he compares two analyses, one of a clay slate, the other of a petrosilex; the first made by M. Regnault, the other by Saussure; and that he has found them very nearly identical. The petrosilex may, therefore, proceed from the slate; but the word *petrosilex* does not well indicate the rock M. Fournet had in view, and he proposes to give to this class of rocks the name of *Thermantide*, a denomination used by Haüy in a more restricted sense, to indicate rocks altered by non-volcanic fires.

The *kaolinic* disorganisation which the slates are liable to undergo, like all the plutonic rocks, is still another character which, in the opinion of our author, has been left upon them by the heat to which they have been exposed.

M. Fournet thinks that, in certain cases, the mica is a sign of metamorphism, for the *denomination mica does not apply to a definite body ; it expresses an equivocal physiognomy, and not settled features ;* and this body must be formed at the expense of the clay-slate, and by associating the latter with the substances brought by the veins.

It is nearly the same with the talcs and chlorites which, in the Alps, form complex and confused rocks by associating with other minerals, as well in respect to their composition as in reference to their situation and origin.

On this occasion, the author reverts to some considerations long previously established by him, relative to some terminological errors which have been introduced in reference to the crystalline slates of the Alps. They are almost always spoken of as talc schists, a name which would lead us to believe in the great superabundance of magnesia, while in this locality they are nothing else than what they are everywhere else, rocks essentially aluminous.

With regard to the modification of the limestone; its change into saccharoidal marble by the action of a high temperature, is, no doubt, most remarkable. The author, however, does not stop here; because this transformation has been for a long time demonstrated geologically and chemically; but he considers the formation of garnets, and idocrases, which are developed in the same circumstances as the saccharoidal limestone. In truth, according to the numerous indications given by M. Fournet, almost everywhere this limestone is accompanied with idocrase, and the formation at Martigny is only one proof more of the law to which the formation of this latter mineral is subject.

We know that M. Mitscherlich has obtained garnet artificially, and that idocrase, as well as another crystallization of the same kind, has been found by MM. Hausmann and Bertheir, in smelting-houses. The garnet and idocrase are nothing else, according to many mineralogists, than the same substance appearing under two different forms. However this may be, there is no ground for separating these two species, in respect to their formation. It is evident that they are formed when there are siliceous rocks in presence of calcareous rocks, in suitable circumstances of temperature and pressure.

These considerations, according to M. Fournet, are of interest in regard to the study of metalliferous deposits, by establishing the influence of matalliferous veins on their walls, a study which is of the highest importance with respect to its practical and industrial bearings.*

Some Remarks on the High Temperature in the United Mines.

By Robert Were Fox.†

The temperature of some of the deeper parts of the United Mines has long been observed to be remarkably high; and it has greatly increased with the increasing depth of the excavations.

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* Biblioth. Univers. de Genev. Supplement, No. 6, p. 203.

† Read, 3d September 1846, to the Royal Cornwall Polytechnic Society.

Captain Youren, one of the agents of the mine,* informs me, that near the eastern extremity of the deepest level, on the "*middle lode,*" there is a spring or jet of water, discharging about 94 gallons a minute, at the temperature of 106⅓° Fahrenheit.† This level is about 250 fathoms below the surface, and about 200 fathoms under the level of the sea. The "lode" has an underlie or dip of about 2½ feet in a fathom towards the north, and the water flows from its *northern or upper wall;* whilst from the opposite side, or *southern wall* of the lode, at the distance of only 3½ feet, there is another spring, discharging 30 gallons of water in a minute, at the temperature of 97¾° Fahr. The air near both these springs was found to be at 104¼° Fahr.: and "*killas*" is the only rock which has been seen within 30 fathoms of them. Granite occurs at a considerable distance westward of the place; and two "elvan courses" traverse the mine in nearly the same east and west direction as the lode.

I have found that ¼ of a pint of the water from the warmer spring contained 15 grains of saline matter, consisting of muriate of lime and common salt, in about equal proportions, with a trace of sulphuric acid, probably combined with lime. In the same quantity of the cooler water, only 10½ grains of muriate of lime and common salt were found, the latter in less proportion than the former; and in this water also there was a slight trace of sulphuric acid. In both instances, the water was clear, saline to the taste, and without any metallic salt.

It may, I think, be inferred from the saline contents of these springs, that they have a common origin or source; whilst their high temperatures indicate their having come from a considerable depth, and the quantity of water they discharge, that the lode, or rocks beneath, must be very pervious to it. In these instances, at least, there are no grounds for supposing that any chemical decomposition of the sulphur ores in the lode has caused the high temperature of the water, or contributed to it in any degree, seeing that it contains no metallic, and scarcely any sulphate salt.

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* This mine (for it is one concern) continues to produce abundance of copper ore. It is situated in the parish of Gwennap, about 8 miles to the northward, or almost NNW. of Falmouth, and is several miles from the sea.

† The thermometer employed has been carefully compared with a standard one, and found to be ¼ of a degree too high; so that this small amount must be deducted from the results, making them 106·08°, 97·5°, and 104 , respectively.

The difference in the heat of the two springs may perhaps, in part, be attributed to the tendency of the warmer currents to rise towards the upper wall of the lode; and still more to that of water at a much lower temperature passing from superior strata down upon the inclined surface of the lower wall, where, mixing with the water rising from below, the temperature becomes modified, as well as the proportion of the saline contents.

It cannot be doubted that ascending and descending currents of water, more or less copious, and at different degrees of temperature, abound in the veins and fissures of the earth, and often at the junctions of different rocks, and that they must have a great influence in modifying the subterranean temperature, and in different degrees in different places.

Common salt is of rare occurrence in our mines; its presence in the water in question cannot well be attributed to the flowing of sea-water into the excavation, in consequence of its local or direct pressure; for if some miles of distance from the coasts did not render this highly improbable, the considerable streams at very high temperatures, and very constant too (as appears from observations made at different times), are facts not consistent with such an explanation. If the subterranean jets of water were caused by the inroads of a neighbouring sea, we should expect to find them at comparatively low temperatures, aud these diminishing in proportion to the duration and amount of the influx.

The salt may, however, have been derived from the ocean, in consequence of the latter penetrating into the earth at its greater depths, or even at its lesser ones, which, under different given circumstances, it may be supposed to do. In either case, the salt water would, from its superior specific gravity, have a tendency to descend through the heated and less saline water in the veins, fissures, &c., where the fluids becoming gradually more or less mixed and extended in different directions, might ultimately appear in some of our mines, brought up, perhaps, in the largest proportions, by the upward tendency of the more heated currents of water. —*The Fourteenth Annual Report of the Royal Cornwall Polytechnic Society*, 1847.

On the High Temperature of the Water at the United Mines.

By Captain W. FRANCIS.

Whitehall, 2d September 1846.

DEAR SIR,—In fulfilment of my promise to you yesterday, I now give you some facts which have come under my notice relative to the temperature of water in our mines, especially in reference to the great difference in the heat of water issuing from different parts of the same level; remarkable instances of which are now observed in the United Mines.

The United Mines on the south are traversed from west to east throughout their whole extent by two large elvan courses, which are of considerable thickness, and whose direction is almost parallel with the principal lodes. At the surface, the distance from the south lode to the first elvan course is about 200 fathoms, and to the second about 230 fathoms. By the dip or underlie, northward, of the elvan courses being so much faster than that of the lodes, the former intersect and pass through the latter at depths which vary in different parts of the mines; but the 155 fathom level may be taken as the average depth at which the south lode passes through the first elvan course; the middle lode passes through it at the 170 fathom level; and the north lode at the 194 fathom level; whilst the second elvan course, as yet, has only been seen at the 220 fathom level, where the south lode is in it, and the other two lodes can only be intersected by it at a greater depth. These elvan courses are distinctly traceable for several miles, and are so jointed and open, especially near the surface, as to become ready conductors of large quantities of water; and to this circumstance is to be mainly attributed the unusual quantity of water which we have to contend with in these mines.

The quantity lifted being, in the dry season, about 1400 gallons per minute, and, in the rainy season, about 2400 gallons. The greatest difference in the temperature of the water is when we have most to lift, and the mean temperature of the whole is then at the lowest. In every instance the water issuing from the south side of the level is colder than that from the north side, and the water from the south lode is colder than that from the middle and north lodes. The elvan courses, as I have before said, are, at all seasons of the year, the channels through which a considerable portion of the water is conveyed into the mines, and I believe nearly all the additional water in the wet season is brought in by them. This water finds its way into the mines rapidly, and without having passed through the lodes to the same extent as that portion of the water which is always flowing, and which may be considered as the permanent source of supply to the engines.

My remarks in reference to the manner in which the elvan courses intersect the lodes, will at once shew you how any water passing through them will, in every instance, make its appearance on the south side of the levels, and that the south lode will drain a larger portion of the water thus brought into the mines than either of the others; hence it is clear that the temperature of the water depends on the time which it requires to pass from its source to the different parts of the mines, and also to the medium by which it is conveyed. Several circumstances exist to shew that what I have termed the permanent supply of water to the engines, must be derived from sources at a considerable distance from the mines, and that its course through the lodes is at a depth from the surface little short of that at which it comes into the shafts, and which varies from 200 to 260 fathoms.

I am aware that it may be urged that, in other mines, when a difference in the temperature of the water is observed, the same causes to produce it do not exist as I have described in the United Mines; but, notwithstanding such mines may not have large elvan courses running through them, it will be found that other means of letting down water freely do exist, such as large cross-courses, changes of strata, &c.; and the water so discharged into the mines will be found to be of lower temperature than the usual coming stream.

Having endeavoured to account for the difference in the temperature of the water, I should be glad were I able to advance some facts illustrative of the source of heat itself; but although I have watched narrowly the various circumstances under which the highest temperatures have been observed, I am still in some doubt as to the causes by which they are produced.

It is a commonly received notion that the highest degrees of temperature are indications of large bodies of ores being contiguous, if not actually open to sight; but at the United Mines, and in others also, the lodes in those levels where the heat is greatest, are almost without ores, and both the lodes the rock about them are hard and very compact. At the same time I admit, that such lodes have been found, somewhere, to contain large quantities of copper ore; but my object in making the preceding remark is rather to shew that the presence of great heat is not to be considered an infallible indication of large masses of ores being near at hand.

It should also be observed, that those lodes which yield the most copper ores generally hold considerable quantities of other minerals, such as iron-pyrites, arsenic, &c.; and it becomes a question, whether these may not account for much, if not the whole, of the heat in the mines.

That most of the lodes, and especially those which have yielded large quantities of copper ores, have, at some period, been under the influence of great heat, there can be no doubt, and to such an extent as to fuse their contents; and I see no reason for believing otherwise than that intense heat still exists in them at a distance from where they have been penetrated: but it should be remembered that the symptoms indicating the action of heat are not so frequent or strong in the deepest parts of those mines which have been worked to the greatest depths, as they are at higher levels, and especially near the surface, where gosson, at the depth of a few fathoms, or even a few feet, usually exhibit this fact in the clearest manner. I have had numerous opportunities of witnessing the fact just named in the deep mines in the Gwennap district, and in almost every case the lodes at the lowest points are composed of hard compact quartz, almost free from the presence of minerals, and yet the water issuing from those parts is of the highest temperature, which certainly strengthens the opinion that the source of heat is from a still greater depth; and its effects not being so evident in those places may be accounted for by the absence of those substances in the lodes on which the action of heat would manifest itself. But whilst it is admitted that the temperature increases with the depth, it should be observed, that it is not found to be the same at the same depth, even in parts of the same mine which are only a few fathoms' distance from each other. In different mines in the same locality, and in precisely similar formations, the result is often very different. As instances of this, I will refer to the Consolidated Mines, which adjoin the United Mines, and the lodes in each bear the same direction, and run parallel to each other. The former have been worked to the depth of 316 fathoms from the surface, whereas the latter have only been worked 260 fathoms, or 56 fathoms less than the first. The highest temperature in the Consolidated Mines has been 98° Fahrenheit, whilst in the United Mines it is 106°. Again at Tresavean, the depth of which is 320 fathoms from the surface, the highest temperature yet observed is about 95°. It should, however, be noticed, that the deepest part of Tresavean is in granite formation, and that the Consolidated and United Mines are in slate.

These circumstances go to shew that there must be other causes producing heat, apart from the mere depth of the mines; and, without being prepared with conclusive evidence as to its origin, or the source from whence it proceeds, I cannot help remarking that the degree of heat is very much increased by its passing through immense bodies of mineralized matter powerfully acted on by galvanic agency.

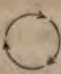
I feel that I have now carried these hasty remarks as far as it is safe to go, without entering upon ground which it would be dangerous for me to venture on; and I will bring them to a close, by expressing a hope that they may serve, in some measure, to elucidate a subject which has engaged a share of your valuable attention, and which must be one of much interest to all parties concerned with our mines.—I remain, &c. &c. WILLIAM FRANCIS.—*The Fourteenth Annual Report of the Royal Cornwall Polytechnic Society*, 1846.

Storms in the Southern Hemisphere.

A paragraph has lately appeared in many of the English and American newspapers, stating, that a vessel called the Charles Heddle, had scudded in circles during a storm for several days. A description of this storm, with a chart of its track, shewing also the manner in which the Charles Heddle scudded in it, has now been published by Mr Piddington of Calcutta, in his Thirteenth Memoir of the Law of Storms in the Indian and China seas.

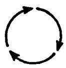
The Charles Heddle is a brig belonging to the island of Mauritius, commanded by Captain Finck, an able seaman. The brig had been originally built as a slaver, and is a very fast sailer.

A copy of the log-book of the Charles Heddle, which was in French, was sent by the master-attendant at Mauritius, Captain Royer, to Mr Piddington at Calcutta; and Mr Piddington having procured the log-books of several other vessels which encountered the same storm, has been enabled to draw up a very instructive memoir.

The Charles Heddle sailed on the 21st February 1845, from the Mauritius, on a course nearly due north. Soon afterwards the weather became thick all round. At night the sea and wind gradually increased, and the vessel began to labour greatly. At 9^{h} 30^{m} P.M., the wind being at the time SE., and having begun to blow very hard, the mainyard went in two in the slings. The mizen and jib were taken in, and the vessel put before the wind, scudding under her fore-sail, fore-top-sail, and fore-top-mast stay-sail, until daylight. Then were struck the main-top-sail yard, and the top-gallant yard. At noon the fore-sail was close reefed, and the brig scudded to the NNW., before the wind at SSE. She was now within the compass of a whirlwind, which revolved like the hands of a watch, thus,  being in the *Southern* Hemisphere, in latitude 16′ 42″ south, and longitude 57′ 45″ east. Scudding round in this whirlwind, the wind from SSE. became south, and continued to the brig constantly veering as she sailed round and round. Thus, in the log-book it is next recorded, that she scudded north, then to NNE., next, NE.; her course changing to ENE., to east, to ESE., to SE., to SSE., and then to south, when the wind in the log-book is marked at north. The log-book goes on to shew, that the Charles Heddle's course was changed from south to SW., to WSW., to west, to WNW., to NW., and to NNW., the wind throughout being marked in the opposite quarter, as she was scudding before it, at twelve knots an hour. Thus she completed her first entire revolution round the vortex of the storm. By this time she was reduced to bare poles, for her sails had blown away. Scudding under bare poles, she continued changing her course from NNW., to north, to NNE., to NE., to ENE., and round again through all the points of the compass, to NNW.; being marked nearly throughout as running at the rate of eleven knots, and the wind always recorded as being exactly in the opposite point to the course she was scudding. In marking this second circuit, it is stated in the column of remarks, that she broached-to several times.

In this manner did the Charles Heddle make five complete circuits, wanting only four points of the compass round the vortex of a storm, by steering always before the wind. Scudding before the wind, the brig was prevented from getting out of the whirlwind as she would have done had she been steered either towards the east, the north, or the south-east.

Allusion is made to this hurricane, and to circuit-sailing in storms, in papers recently published by Mr Redfield in the *American Journal of Science*. He says :—

"In the slow progression found in storms of the eastern seas, as already noticed, not only a complete circuit of revolution, but more than *one circuit* might sometimes be made, in a gale by the same vessel, in sailing round the axis of the storm; thus adding another practical demonstration of its revolving character. One such case of complete circuit-sailing I have referred to in 1836. Mr Thom, in his account of the Rodriguez storm of April 1843, has shewn, that the Robin Gray ran once and a half times round the axis of the storm from left to right,  (this being in the *Southern* Hemisphere), till, being thrown on her beam ends, she was prevented from continuing her circuit. In the same storm, the *Argo* made part of her second circuit, scudding round in the gale in the same direction. In like manner the *Margaret* made a circuit and a quarter round the axis, chiefly in the heart of the gale. Several vessels, after once falling out of this hurricane, pursued their course, again overtook it, and plunged into the heart of the storm, where they suffered most serious disasters. It appears probable, and indeed certain, that nearly all the great loss and damage sustained in this hurricane might well have been avoided by a knowledge of the laws of rotation and progression in these storms.

"But the most striking case of circular sailing in a storm, is that of the Charles Heddle in a hurricane near Mauritius, in February 1845, which has been furnished me by Mr Piddington. This was a clipper-built vessel, once a slaver, and was bound from Mauritius to Muscat. It appears from the log, that in her course round and round in the gale, the wind veered five complete revolutions in 117 hours, with an average run of eleven and seven-tenths knots per hour; the whole distance thus sailed being 1373 miles, while the progression of the hurricane at this period *was less than four miles an hour*. The average distance from the gale's axis is estimated at about 45 miles. During this time the vessel made a good course SW. ¾ west, 354 miles only; nearly on the usual course pursued by the hurricanes near Mauritius.

"These are results obtained by Mr Piddidgton, who has already published his twelfth memoir, and who informs me that he is preparing another on this hurricane of the Charles Heddle. In his eleventh memoir he has given us an account of two storms, which were nearly contiguous, but on opposite sides of the equator, and *revolving in counter directions* ↻ ↺ each according to the laws of rotation and progression of its own polar hemisphere."

Some idea may be formed of the figure described in one of Mr Piddington's charts, of the circuits sailed over by the Charles Heddle, by taking one end of a coil of rope, and opening it out upon a deck or floor, in such a manner as to form with the rope five distinct loops. This will also shew the direction in which the brig scudded, as ropes are coiled from left to right.

Mr Piddington is of opinion, that storms do not always dilate in their progress, but, on the contrary, that they sometimes contract; and that when they contract, the squalls of wind partake of an involute direction, drawing ships which scud nearer and nearer to the vortex. Such an action as this may, he thinks, have drawn the Briton and Runnymede transports towards the same point in their storm in the Bay of Bengal.

These two transports coming from different points, the one from England, and the other from Australia, encountering the same storm, both became involved in the vortex, and were cast on one of the Andamen Islands, within a few yards of the same spot, and at the same time.—*Bermuda Royal Gazette*, Sept. 15, 1846.

On the Erratic Phenomena of Scandinavia, in reference to M. Durocher's Memoir on that subject. By M. CH. MARTINS.

28

When M. Durocher published his first memoir on the erratic formation of Scandinavia,* I had the honour of pointing out to the Society how readily the facts observed by this traveller admit of explanation, by supposing the ancient extension of the glaciers of Sweden and Norway.† It is the same with the present, in which the author is obliged to confine himself to the relation of what he has seen, without attempting to explain the greater part of the geological phenomena, which he mentions in connection with the hypothesis of diluvian currents.

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* Bulletin de la Société Géologique, 2d Series, t. iii., p. 45.

† Ibid., p. 102.

Direction of the Striæ.

M. Durocher's new Memoir commences with an account of the direction of the striæ in the south of Scandinavia. He gives a map of these directions, in which he fills up some blanks left by MM. Seftsroem,* Boethling,† Siljestroem,‡ and Keilhau.§ But these gaps are so few in number, that by uniting the three charts to which we refer, it will be easy to construct that of M. Durocher.

The general result of the author's new researches is, that the direction of the striæ are not parallel, neither in the different provinces of the peninsula, nor in any limited locality; but that they often form more or less open angles with each other, both in the same country, and in very remote provinces. MM. Siljestroem and Keilhau had previously arrived at the same result, without endeavouring to seek for an explanation of it. This M. Durocher has attempted, but, perplexed with the multiplicity of these directions, he is compelled to refer them to numerous systems, or to different groups. Thus, for example, he found that in the neighbourhood of Christiania and Lake Mioesen, the greater part of the striæ have a certain orientation. Around Areskutan, 380 kilometres to the north, he again found others nearly parallel to the first; and without having visited the intermediate region, or taking into account the striæ which, in these countries, form considerable angles with those he had chosen, he concluded that one and the same grooving system (such is the kind of expression he employs), extended from 63d to 59th degree of latitude, and proceeded from the north to the south of Norway. The author gives a similar account of the striæ which present a similar orientation at Gefle, on the shores of the Gulf of Bothnia, and in the neighbourhood of Gothenburg, a town situate on the North Sea. This mode of proceeding appears to me altogether arbitrary; for we might quite as easily form other groups completely different from the first, and which rested on as slender grounds. Why, for example, should we not consider as forming part of the same system, the striæ running from NNW. to SSE., in the vicinity of Areskutan, in Norway, and those which cover Finland, inclining in the same direction? In order to justify the establishment of these different groups, it would be necessary to prove, first, that the striæ running in the same direction, are the effect of a uniform and uninterruped agent, such as a current or other mass capable of leaving marks of its passage; now, this M. Durocher has not done. Neither does he allow himself to be arrested by the directions of striæ which intersect these different groups at angles more or less open. Thus, the prolongation of the great system running from NNW. to SSE., from lake Oestersund to lake Maelar, near Stockholm, that is to say, from 63d to 59th degree of latitude, is at a right angle with the direction of the striæ of the island of Gottland. The striæ running from the N. to S., in the south of Sweden, are intersected, under an angle of 45°, by those in the neighbourhood of Cimbrishamn. M. Durocher, besides, leaves out of his ten systems of erosions, all the striæ whose direction does not conveniently agree with them; those, for example, which radiate round the Suletimten group of mountains, in the lower part of the Sognefiord; those in the neighbourhood of Sneehætten, Ekesjoë, Wexioë, Carlskrona, Cimbrishamn, the island of Gottland, &c., and notwithstanding all these liberties, the author concludes without saying a single word on the force of facts which demonstrate that it is not water which has grooved the striæ of which he speaks; for it would be necessary to admit ten currents, perfectly rectilinear, simultaneously or successively directed towards the four cardinal points, without deviating from a straight line; a supposition inadmissible and contrary to all that we know of the laws which regulate the courses of water, whatever may be their force or volume.

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* Untersuchung über die auf den Felsen Scandinaviens in bestimmter Richtung vorhandenen Furchen.—*Annales de Physique de Poggendorf*, t. xliii., p. 533. 1838. With a map of the direction of those striæ throughout the whole of Southern Sweden, from 56th to 61st degree of latitude.

† Ueber die diluvial Schrammen in Finnland.—*Ann. de Physique de Poggendorff*, t. lii., p. 641. 1841. With a map of the orientation of the striæ in Finland.

‡ Voyages in Scandinavie de la Commission du Nord.—*Geographie Physique*, t. i., p. 193. With a map of the whole of Scandinavia, and the mean direction of the striæ in Norway, drawn by M. Bravais, according to the indications of the author.

§ Reise fra Christiania til den Oestlige Deel af Christiansands stift i sommeren, 1840.—*Nouveau Magasin pour les Sciences Naturelles de Christiania*, t. iii., p. 169. 1841–42. With a map, shewing the orientation of the striæ in the western part of the Gulf of Christiania.

The extension in ancient times of the Scandinavian glaciers, easily explains the varied directions of the striæ in the different regions of the peninsula. Their very parallelism would be an objection against the glacial hypothesis, admitting of no reply. Let us, in the first place, examine this phenomenon in Switzerland, where it has been studied with the greatest care, and by individuals possessing the most profound knowledge of the progress and effects of glaciers now existing. In Switzerland, as in Sweden, the striæ assume the most varied directions; it is easy to account for this. The plain which separates the Alps and the Jura, was formerly filled with glaciers, which descended by the principal basons of rivers, whose source is in the Alps; the Arve, Rhone, Aar, Reuss, Limmat, and Rhine. But each of these glaciers, carrying along with it the rocks which characterise it, had a different direction. Thus, while the glacier of the Rhone formed striæ in the Valais, between Sion and Martigny, running from ENE. to WSW., the ancient glacier of the Rhine passed over the moutonnéed rocks, which bear the ruins of Baerenburg, near Andeer in the Grisons, in the direction from SW. to NE.; and the glacier of Val-Montjoie grooved the polished rocks in the neighbourhood of Nant-Bourant, at the foot of Col du Bonhomme, in the direction from SSE. to NNW. Nay, more; the direction of the striæ engraved by a glacier is not the same at different parts of its progress. In the environs of St Maurice, for example, the glacier of the Rhone has left striæ on the limestone, running from SE. to NW., while above Vevay, the gompholite is striated nearly in the direction from E. to W. At the mouth of the valley of Chamonix, in front of the village of Ouches, the glacier of the Arve has formed striæ running from ESE. to WNW.; while, on issuing from the valley, we find, above the gorge of Montets, furrows many metres in length, the direction of which is from SSE. to NNW. Besides these facts, the striæ traced by the tributary of a glacier often cut the striæ engraved by the principal glacier under very wide angles. Thus, the glacier of Argentière, which descended by the Col de Salvent into that of the Rhone, has left, as the marks of its passage, striæ almost perpendicular to those of the latter. The furrows of the valley of Urbach cut those of the valley of Hasli, &c. &c.

I could multiply these examples, but I prefer to derive some instances from existing glaciers, in order to avoid the objection of some of the opponents of the ancient extension of the Swiss glaciers, who deny, notwithstanding the identity of the facts, the glacial origin of the striæ found in the plain between the Alps and the Jura. Let us conceive, for an instant, that the glacier of the Aar, or the Mer de Glace of Chamonix had disappeared. We would find in the valley they occupied, longitudinal striæ traced by the principal glacier, and transverse striæ formed by the tributary glaciers which cut the first under angles more or less acute. We would observe ascending striæ at the contraction of the valleys, and striæ crossing each other at the point where the two glaciers met; in a word, all that exists on a large scale in the Swiss plain and in Scandinavia. The phenomena and the explanation would be the same, only they would apply to a greater surface of country.

Accordingly, in the plains of Switzerland, as in those of Scandinavia, the striæ are found running in various directions; but in Sweden, as in Helvetia, these striæ are identical in every respect with those engraven by existing glaciers; and I do not think that we can, consistently with good logic, ascribe to one agent effects identical with those which an agent totally different produces every day under our own eyes.

In order to complete the proof that M. Durocher's system is arbitrary and fruitful in erroneous consequences, let us apply it for an instant to Switzerland. We find on the southern shores of the lake of Brienz, striæ running from NE.

to SW. In the Valais, there are striæ nearly in the same direction, such as we again encounter in the valley of Chamonix, between Prieuré and the village of Ouches. According to M. Durocher's principles, all these striæ having the same orientation are united, and form a system of erosion, which, commencing in the Oberland, passes above the high chain of the Bernese Alps, crosses the Col de Balme, and extends to the foot of Mont Blanc; while, in reality, these striæ belong to three different glaciers; that of the Aar, that of the Rhone, and that of the Arve, which had nothing in common with each other, excepting that their directions were parallel at one point of their progress. According to this view of the subject, we should be forced, in order to be consistent, to consider the striæ lying in the same direction in the Alps, Vosges, and Pyrenees, as forming part of one system of erosion, over a space of four degrees of latitude, like that which many of M. Durocher's systems of striæ comprehend; while the latest evidence goes to prove that these striæ have been engraved by glaciers descending from mountains into valleys, and not by a general agent proceeding from the north to the south, or from the east to the west.

Faithful to his principle of assuming the direction of the striæ as the sole guide in studying the line of route followed by the agent which traced them, an observer, proceeding on this method, would be forced to regard the furrows formed by a principal glacier, and those made by its tributaries, as belonging to different systems. In the valley occupied by the glacier of the Aar,* such a geologist would consider the striæ directed from west to east which this glacier has engraved under our eyes, as forming part of one system, while those of its tributaries, the glaciers of Thierberg, Silberberg, Grünberg, and Zinkenstock, which cut the former at right angles, more or less obtuse, he would regard as belonging to another system.

The plan followed by M. Durocher appears to me, therefore, erroneous, whether we apply it to the hypothesis of glaciers, or to that of currents; for, in either case, we could not take the direction of the striæ as the only indication of the route taken by the agent which traced them, without taking into account all the other marks which might serve to make us acquainted with this direction. The problem is not beyond the means of solution, for M. A. Guyot has figured upon the map the space occupied by the different glaciers which formerly covered the plain lying between the Alps and the Jura. But, besides the direction of the striæ, he has taken into account the mineralogical nature of the transported rocks, their mode of dispersion, whether in a sporadical state, or in the form of lateral, median, or terminal moraines. He has followed these materials from the point of their departure in the Alps to their point of arrival in the Jura, and he has thus been enabled to circumscribe the domain of each glacier with as much accuracy as we can trace the limits of a superficial formation.[*]

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* See the plan of the glacier of the Aar, *Bulletin de la Société Geologique*, 2d series, t. iii., Pl. V., fig. 12. Meeting of 2d March 1846.

Characters of the Transported Deposits in the north of Europe.

In the flat country, this formation presents itself sometimes under the appearance of vast deposits with a flat surfac sometimes under that of terraces rising in steps one abo another, or in the form of elevations of banks or elongat hills, known by the name of osars (the *sandosars* of t Swedes). Among the mountains, it forms irregular masse without any determinate geometrical figure. Instead of e deavouring to discover characteristic marks which might illu trate the nature of these different deposits, M. Durocher co fines himself to a description of their form, and that of th materials composing them, as if that form, and the great or less proportion of sand, pebbles, angular or rounded blocks, were of themselves sufficient to reveal their origin to us. I his first Memoir, he seemed to separate the osars from th other transported formations, and to admit, with Mr Lyell, that their origin is more recent. In the present Memoir, this distinction is not observed, and the author no longer attempts to determine whether they contain shells, and if these shells are identical with those now living in the neighbouring seas, or rather, whether they belong to the icy ocean, or to warmer seas. On no occasion does he make the slightest allusion to the absence or presence of those rubbed and striated pebbles* which are so characteristic of moraines. The result is, that the reader, after perusing this chapter, cannot form the slightest idea of the nature and origin of these deposits, so different from each other. However, before M. Durocher's publication, the results of MM. Hisinger,† Lyell,‡ Keilhau,§ and, at a later period, those of MM. Forchammer,‖ Loven, and Desor, have thrown some light upon this chaos.

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[*] In his first memoir, M. Durocher had ascribed the sinuous canals, ramified and anastomosing with each other, which he had seen on the shores of the sea, to diluvian currents.[*] In the present, he remarks on the contradictions in the explanations which have been given of them by MM. Agassiz,[†] Escher de la Linth,[‡] P. Schimper,[§] and myself.[‖] Now, all the four of us have ascribed the sinuous and anastomosing canals observed by M. Durocher to the action of water; only MM. Agassiz and Escher have considered the striæ engraved *in the interior* of these canals as the striæ of glaciers. M. Schimper ascribes them to the action of waves. For myself, I have not pronounced an opinion on the origin of these striæ, which were known to me, neither by specimens, nor detailed description, nor by accurate drawings. Accordingly, in my first reply, I have not spoken of these striæ, and, in my second, I have expressed myself thus: "With the rest of M. Durocher's argument, supposing what I have not said, namely, that the striæ engraved in the interior of sinuous canals have been produced by glaciers, I shall not occupy myself." It will thus be seen that we agree as to the origin of these canals, only M. Schimper differs from MM. Agassiz and Escher in the explanation of one of the details, the striæ in their interior.

[*] See the form of these sinuous canals, *Bulletin de la Société Geologique*, 2d series, t. iii. Pl. I., figs. 1, 5, and 8; that of the striæ of glaciers, Pl. II., figs. 3, 4, 5, and 6.

[†] *Comptes Rendus de l'Academie des Sciences*, t. xxi., p. 133. 15th Dec. 1845.

[‡] *Bulletin de la Société Geologique*, 2d series, t. iii., p. 236. 19th Jan. 1846.

[§] *Comptes Rendus de l'Academie des Sciences*, t. xxii., p. 43–5. 5th Jan. 1846.

[‖] *Bulletin de la Société Geologique*, 2d series, t. iii., p. 111. 15th Dec. 1845.

Under the name of transported deposits, M. Durocher has united, in the same denomination, three very distinct formations:—1*st*, The ancient moraines, which he describes (page 64) as "deposits formed in the ravines of high mountains." He himself recognises the analogy of their form to that of the moraines of existing glaciers. Unfortunately, he has not ascertained whether they contain rubbed or striated pebbles. Negative or positive, this character was of such importance, that I am forced to believe that it was completely unknown to him; for he must have searched for these pebbles with the greater eagerness, as their absence would have been a decisive proof that these masses are not moraines, but deposits entirely aqueous.

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* These pebbles had already been described in 1842, in the *Edinburgh New Philosophical Journal*, p. 223, and in the *Bibliotheque Universelle*, t. xli., p. 125.

† *Anteckninger i Physik och Geognosie*, t. iv.

‡ On the proofs of the gradual rising of the land in certain parts of Sweden. *Phil. Trans.*, 1835; and in French, *Memoires de la Société des Sciences Naturelles de Neuchâtel*, t. i., p. 200.

§ Nyt Magazin for Naturviderskaberne, t. iii., p. 169. 1842.

‖ The Athenæum, No. 987, 26th September 1846, p. 1003.

2*d*, When the transported formation presents itself under the form of even or terraced surfaces, it shews an imperfect stratification, and its exterior shape indicates the action of water. But these formations are almost wholly composed of that layer of mud, sand, and gravel (*moraine profonde*), which is found in the lower part of all glaciers, afterwards arranged by the waters. The proof of this is to be found in the presence of large pebbles, or rather blocks striated by the ice, blocks which exist neither in the sea, nor in lakes, nor in torrents; for, not only does transportation by water not striate pebbles, but it effaces the striæ, as any one may convince himself by examining any of the torrents which issue from the glaciers of Switzerland.

In his travels in Southern Scandinavia during this summer, M. Desor found striated pebbles in Denmark in a transported formation, with an even surface, across which the railway from Copenhagen to Roeskild passes, and in the neighbourhood of Kioege, on the coast of Seeland, to the south of the capital. In Norway he again found them in a transported formation of terraces at Sorgenfry, near Christiana, on the road which leads from that town to Krogleben, and in the muddy deposits on the sides of the Drammen, at its outlet into Drammen-Fiord. In Sweden he saw striated pebbles in the same kind of formation, particularly in the loamy terraces of Berg, near lake Wettern.

These striated pebbles are not the only signs which disclose the glacial origin of these deposits; they likewise inclose marine shells. A great number of these have no analogues in the neighbouring seas; they are, according to M.

Lovén, arctic species now banished from the icy seas; b which lived in the North Sea at the period when its coast by sinking, plunged into the ocean the moraines previously arranged by the diluvian torrents arising from the melting of the glaciers.

3*d*, The osars (*sandosars* of the Swedes). These hills, with such characteristic forms, belong to an epoch very different from that of the erratic transported formation of which we have spoken; they are one of the numerous proofs of the immersion and emersion of the Scandinavian coast. MM. Al. Brongniart, Hisinger, and Lyell, have found in them shells now living in the Baltic (*Tellina Baltica, Cardium edule, Mytilis edulis, Littorina crassior, L. littoria, Neretina fluviatilis*), at heights varying from 9 to 24 metres above the level of the Gulf of Bothnia. These osars are, therefore, the work of the sea during the period of the immersion of Scandinavia. They are true downs (*Revler* of the coasts of Jutland), with a stratification not horizontal, formed by the surge during stormy weather which passed over these ramparts, and deposited in them the pebbles and sand which it raised from the bottom of the sea.* The pebbles composing these osars are never striated; they could not be so; for, even if they were striated before, the striæ would have been speedily effaced by the friction to which they were subjected by rolling on the beach. The osars are almost always covered with angular erratic blocks, and the most probable opinion is that which attributes their transport and deposition to rafts of floating ice. Another circumstance still proves the recent origin of osars, namely, that they sometimes lie above deposits of peat, the origin of which may be contemporary with that of glaciers, but cannot be posterior to it. Besides this, boats have been found in them, and a house buried to the depth of 19 metres, with charcoal still in the fireplace.† M. Durocher, therefore, is in error when he terminates his memoir by saying (p. 64), "The partisans of the glacialist school consider these deposits to be ancient moraines." Already in my first reply,* I had insisted at length on the action of waters which have arranged the materials of moraines, or have so deposited them as to form terraces or osars. In Switzerland, the aqueous debâcle, arising from the melting of glaciers, has produced analogous effects; but in Sweden, the phenomenon is complicated by the successive immersion and emergence of a great part of the shore, before and after the ancient extension of glaciers.

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* Ueber Geschiebebildungen und Diluvial Schrammen in Danemarck, von G. Forchammer. Annales de Physique de Poggendorf, t. lviii, p. 609; also Edinburgh New Philosophical Journal, vol. xxxi., p. 61, 1841.

† Lyell, On the Proofs of a gradual Rising of the Land in certain parts of Sweden. *Phil. Trans.*, 1835, p. 8.

Reply to M. Durocher's Objections to the idea of the Ancient Extension of the Glaciers of Scandinavia.

The Scandinavian peninsula is a mixed country, composed of mountains, valleys, and undulating plains. This country has not preserved, nor does it yet preserve, a constant level above the sea; it has been sunk under and raised from the water once or many times. It has, therefore, been subjected to very complex actions; the action of glaciers more extensive in former times than now,—the action of water arising from the melting of these glaciers,—the action of the sea at a greater or less distance from its present shores. It would, therefore, be very illogical to explain all by a covering of ice, or to desire to account for all the facts by one or more diluvial currents. Now, M. Durocher always imagines that the advocates of the ancient extension of glaciers ascribe all the erratic phenomena of Scandinavia *exclusively* to the action of ice. He has in this way created a convenient adversary whom he can easily combat. But let us look to the facts. The author supposing, from the direction of the striæ, that one and the same agent has furrowed Sweden from the 63d to the 59th degree of latitude, calculates the general inclination of the ground for a length of 450 kilometres; he finds it to be two minutes only, and concludes that a glacier could not move over so slight an inclination. But observation has decided nothing as to this point; the glacier of the Aar moves over a bottom which inclines only 1° 30′, and M. Hopkins has proved experimentally that a block of ice, melting at its lower surface, slides over a slope of 40′.* In making this objection, M. Durocher supposes, along with Mr Forbes, that the movement of glaciers is nothing else than that of heavy bodies.† If he had studied this movement, he would have found that such is not the case. The glacier of the Aar moves on a very unequal bed, sloping 1° 30′, an inclination amounting almost to nothing, and on which a solid body would remain stationary; for, according to M. Morin, the two bodies which slide most easily on one another are copper and greased iron; but the former does not get into motion till the inclination reach 4° 35′. In a heavy solid body, viscous or liquid, the lower extremity advances with a degree of quickness sensibly equal to that of the upper parts. In a glacier, the movement becomes slower towards its lower extremity. Thus in the present year, whilst at the middle portion of its length, the glacier of the Aar had traversed a space of 75 metres in 396 days, it had advanced only 41 metres near its extremity. The influence of the slope is even so slight on the progress of glaciers that we cannot compare it to the sliding of heavy bodies. M. Desor has already shewn this;‡ M. Dollfus and myself have confirmed it. In fact, while the glacier of Grünberg, one of the tributaries of the Aar, had advanced 2′·22 in 17 days, the latter had advanced 2′·94; and yet the surface of the first mentioned had an inclination of 30°, that of the second only 3°.§

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* Bulletin de la Société Geologique, 1845, page 122 and following.

The second difficulty which M. Durocher advances against the ancient extension of glaciers in Scandinavia, is, that they cannot traverse planes inclined to opposite slopes. In order to be convinced that this objection is not real, we have only to go to Switzerland and examine the profile of some glaciers, where we see the rock on which they rest. We will perceive that all the glaciers pass over planes inclined to opposite declivities. I shall content myself by citing the glaciers of the posterior Rhine, Alalein, Tschingel, and Grünberg. The ancient glaciers of Switzerland present still more striking examples of this mode of progression. I shall recall to M. Durocher's recollection the succession of hills which he must have seen in descending the valley of the Aar; 1*st*, the two Baerenbühl; 2*d*, the groups of moutonnéed rocks which we cross in going from the Aar to the hospice of the Grimsel, and which form the *contrefort* of Naegelisgraeth; 3*d*, the Spitalnollen; 4*th*, the Kirchet, which are covered on all sides with striæ characteristic of the action of glaciers, identical in every respect with those which the Aar is engraving in the present day under our own observation.

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* On the Motion of Glaciers. Philosoph. Mag., vol. xxvi., p. 4, 1845.

† Comptes Rendus de l'Academie des Sciences, t. xxiii., p. 209, 27th July 1846.

‡ Ibid., t. xix., p. 1303, 9th Dec. 1844.

§ Ibid., t. xxiii., p. 823, 26th October 1846.

I was astonished, I must confess, to find the following remark among the objections to the ancient extension of glaciers: "In the country of Areskutan," says M. Durocher, "the erratic agent must necessarily have had an ascending motion; it has set out from a lower country to ascend to another 222 metres higher than its point of departure; it is not, therefore, a glacier which has engraved the striæ of Areskutan." This difficulty is indisputable; but it appears to me, that if it be difficult to admit that a glacier can raise raise itself up a slope, it is still more difficult to conceive that a diluvian current could flow up one; and if we were forced to choose between the two suppositions, there is no one who could not decide for the glacier. In truth, it has not been *demonstrated* that a glacier cannot ascend a declivity by resting itself against an obstacle at the bottom, whilst water cannot move against the laws of gravity. This alleged objection, therefore, against glaciers is only a local problem, of which M. Durocher has been unable to find the solution. There is one, however, which I have endeavoured to give, in favour of currents as well as of glaciers. The coast of Norway, as is well known, is subject to considerable oscillations; and we may affirm, that, in the glacial epoch, the relative levels of the sea, of the coast, and of the interior of the country, were not the same as they now are. Everywhere in the Gulf of Christiania, and elsewhere, the striæ are continued under the sea. It is, therefore, extremely probable, that at the era of glaciers the coast was more elevated than at the present. In fact, it is necessary, in order that glaciers might slide along the bottom of the sea, that its temperature should be *constantly* below zero; for, if the temperature of the latter were maintained above the freezing point during several months of the year only, the glacier would float upon the surface of the sea, and not striate its bottom. I think that I have established this point in my observations on the glaciers of Spitzbergen.* During the glacial epoch, it is, therefore, probable that the coast was higher than at present, for we must suppose it to have been more elevated before all the striæ could be submerged. But since it has undergone remarkable oscillations, as is proved by the balani of Udevalla, seen by M. Brongniart,† 60 metres above the sea; the serpulæ fixed on polished rocks between Christiania and Aggersbach;‡ and above all, by the bed of clay and shells (*Skalenschicht*) described by M. Keilhau, and which are found to the height of 188 metres above the sea, and 12 myriametres from the coast.§ The oscillation of the coast of the peninsula is, therefore, a fact mathematically demonstrated. Let us now see whether the portion of the Norwegian coast, of which M. Durocher speaks, presents any marks of these changes of level. Areskutan is situate under the same latitude as Drontheim, but 10 myriametres into the interior of the country. Now, in the Beistadfiord, 11 myriametres to the north of Drontheim and Areskutan, M. de Buch|| found, at a height of 140 metres, marine shells, which he considers the same as those of the deposit near Drammen, the greater part of them now living in the North Seas. These are, *Mytilus edulis*, *Cardium eduli*, *Mya truncata*, *Venus Islandica*,

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* Bibliotheque Universelle de Geneve, July 1849; Bulletin de la Société Geologique, Meeting of 4th May 1840; and Edin. New Philosophical Journal vol. xxx., p. 284, 1841.

† Notice of the blocks of rocks found in transported formations, *Annales de Sciences Naturelles*, t. xiv., 1828.

‡ Scheerer, Beytraege zur Kentniss des Seftstroem'schen Frictions Phenomen. *Annales de Physique de Poggendorff*, 3d series, t. vi., 1844.

§ Daubree, Note on the Erratic Phenomena of the North of Europe. *Bulletin de la Société Geologique*, t. xiv., p. 574, 19th June 1843.

|| Reise durch Norwegen und Lappland, t. i., 251.

&c. A little to the south of Drontheim, at the entry of the Joergenfiord, latitude 62° 20′, M. Keilhau* observed terraces rising 200 metres above the ocean. Thus, then, the coast of Drontheim, which corresponds to the mountain Areskutan, has been subject, like the rest, to considerable oscillation, and we cannot infer, from its present height, what was its elevation at the period when the striæ were produced.

The difficulty started by M. Durocher is not, therefore, without a solution, and all his calculations on the ascending motions of a glacier are without foundation, since it is all but demonstrated that, at the glacial era, the relative levels of the sea, the coast, and the interior of the country, were not the same as now. It remains to be determined, whether the mean direction of the striæ is really that which he describes; for I must observe that M. Siljestroem, who visited this locality before M. Durocher, mentions only striæ,† directed from north to south, and others from east to west; and none of those running from 1′W. to 1′E., on which M. Durocher founds his reasoning.

M. Durocher's fourth difficulty is one of a similar nature. He cannot comprehend how a glacier could cross obliquely the deep depression of the Gulf of Bothnia, and then ascend the hills of Finland. I can still less understand how a current could do this; in fact, a current from the NW. towards this gulf, would have evidently deviated on entering this depression, and would have followed its direction, which is from NE. to SW., in the same manner as a river which enters a valley, immediately takes the direction of that valley. But we may conceive, how a mass of ice could traverse a depression obliquely. The glaciers of Switzerland present us with numerous examples. We must remember, that the maximum specific gravity of the ice of glaciers is, according to M. Dollfus' experiments, to that of pure water as 909 is to 1000, a proportion which, with muddy water becomes 883 to 1000. This ice, therefore, partly swims on water; and if a glacier supported its sides on the shore, that would be sufficient to sustain it above a liquid mass. These consequences are verified in nature. The glacier of Aletsch, in Switzerland, rests on the surface of Lake Mœrill; all the glaciers which occupy the bottom of the Spitzbergen bays, advance above the sea, to a certain distance, supporting themselves on the shores of the bay.* For all these reasons, it does not appear to me absurd to suppose, that, by enlarging the proportions of the phenomenon, the glaciers formerly crossed the Gulf of Bothnia, as they have probably traversed the lakes of Switzerland. In this country, we can demonstrate, from the altitudinal limit of the ancient lateral moraines left by these glaciers, that their base did not rest on the bottom of the lake, but corresponded very nearly to the actual level surface of the water.

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* Nyt Magazin fur Naturviderskaberne, 1837, p. 220; and Bravais, On the Lines of the Ancient Level of the Sea in Finmark. *Voyages du Commission du Nord en Scandinavia. Geographie Physique*, t. i., p. 117.

† Carte de la Scandinavie, par M. Bravais. *Atlas des Voyages de la Commision du Nord.*

The portion of his map of the direction of the striæ, which M. Durocher has derived from that of M. Sefström,† presents, on the shores of the Gulf of Bothnia, between Gefle and Oeregrund, arrows running from NE. to SW., as if the grooving force advanced from the sea towards the interior of the country. From this M. Durocher draws a great argument against glaciers, which, he says, cannot rise from the bottom of a basin, like the Gulf of Bothnia, upon the shore.

The objection applies equally to currents; but I shall not discuss it; for I have strong reasons for thinking, that the direction indicated by M. Sefström is not the mean direction, but that he has been led into error by local deviations. The recollections of my journey in 1839, in which I travelled from Gefle to Upsal, crossing this region, recall nothing of this nature.

Such are the objections M. Durocher makes to the ancient extension of the glaciers of Scandinavia. Not to prolong the discussion, I shall not enter upon the enumeration of the difficulties that may be raised against the hypothesis of diluvial currents; my task would be difficult from other causes, for in these two memoirs the author has not come to a conclusion. Formerly, he admitted a single current,* which extended from Spitzbergen to Germany; now, I suppose that he must admit ten different currents, successive or simultaneous,† running towards the four cardinal points, according to the mean direction of his arrows. The author not having explained himself, I prefer to wait till he has himself pointed out the consequences of his observations, and shewn how one or more diluvial currents better account for the facts observed, than the supposition of the ancient extension of glaciers.

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* See my observations on the glaciers of Spitzbergen, *Bibliothèque Universelle de Genève*, t. xxviii.. p. 160. July 1848.

† Annales de Physique de Poggendorff, t. xliii. 1838.

Reply to the Objections to the Ancient Extension of Glaciers among the Alps.

M. Durocher terminates his memoir by drawing a parallel between the erratic phenomena of the North, and those of the Alps, Pyrenees, and Vosges. He admits the analogy of the effects produced; but as he does not take into account the powerful action of the sea during the time of the immersions and emersions of the land of Scandinavia, he carries the analogy, in my opinion, much too far. In the Alps, Pyrenees, and Vosges, the aqueous effects are confined to such as resulted from the melting of glaciers, and the incessant action of atmospheric agents. In Scandinavia, there were, besides these, the powerful effects of the sea, which has covered, at different periods, a considerable portion of the Swedish plain. This part of the memoir contains a multitude of observations and details already known, and objections long since refuted. Thus, for example, M. Durocher draws attention to the fact, that, at the contraction of valleys the striæ are ascending. Let him visit the glacier of the Aar, and he may touch, at the lower part of the promontory which supports M. Agassiz's pavilion, the identical striæ rising 64° from before backwards. He will again meet with the same phenomenon at 8 kilometres distance, at the lower contraction of the small valley of Ræterichsboden, where the striæ are inclined 48°, on a very salient promontory of the left bank, and 7° only on the vertical walls of the right bank. Let him go to the glacier of the lower Grindelwald, at the defile of Stiergeg; he will find them raised to 45°. Wherever, in short, an obstacle opposes itself to the progress of the glacier, he will see ascending striæ engraved by the ice which rises against the rock. This is one of the phenomena which water cannot produce, for it does not engrave rectilinear striæ, but hollows out sinuous and ramified canals, holes, and conoidal cavities, giants' kettles, &c.

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* Comptes Rendus de l'Acadamie des Sciences, t. xiv., p. 109. January 17, 1842.

† See his Map.

The author then adverts to meteorological considerations, for the purpose of shewing that, even in a climate analogous to that of the polar regions, the glaciers of the Alps could not extend to the Jura. To prove this, M. Durocher supposes that the mean temperature of these regions is − 15°. This is an error; the mean of Spitzbergen, according to Scoresby,* is only − 8°; and this statement is confirmed by Franklin, Parry, and the Northern Commission. He affirms, moreover, that the temperature is, during a very short time, above zero. Another mistake, for the means of the summer months, are the following:—

June	+ 0° 90
July	+ 3° 51
August	+ 2° 88
Mean,	2° 43

Thus, during three months, the thermometer is habitually above zero. The climate of Spitzbergen is not, therefore, so cold as M. Durocher supposes; and yet all the valleys on the western coast are filled with glaciers which reach to the sea. If Spitzbergen had the mean temperature of − 15°, there would be no glacier in that island; for the two conditions essential to the formation of glaciers are, 1*st*, Abundant falls of snow: now we know that these falls are never abundant during severe colds; 2*d*, The melting of these snows, which become penetrated with water, and are then converted into glacier. Now, on the grand plateau of Mont

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* Account of the Arctic Regions, vol. i., p. 358.

Blanc, where the mean temperature is most probably about −9°·1, no glacier is formed; *a fortiori*, it would not form with a mean of −15°, which M. Durocher imagines to be that of Spitzbergen.

Like all the authors who have not specially studied existing glaciers, M. Durocher fancies that their ancient extension presupposes a very rigorous climate. Such is not the case; some very simple calculations prove it with conclusive evidence.*

M. Durocher terminates his memoir with this remarkable concession: "Nevertheless, we must admit, that the glacial theory has an indisputable advantage, that of introducing agents which produce in our own days, but on a much smaller scale, effects analogous to those which have taken place in ancient times; and it is this circumstance which has gained over many adherents of late years; while, in the diluvian theory, the causes supposed to have been in operation do not now perform the same functions as formerly, and, consequently, do not produce, under our own eyes, effects *altogether* similar to those of erratic phenomena; thus we have not observed, that the waters of rivers produce these erosions in form of fine striæ."

This confession is sufficient for the advocates of the ancient extension of glaciers. Their constant study has been to shew, that the greater part of the phenomena ascribed to diluvial currents, are still produced every day by glaciers; they have adopted the method introduced by M. Constant Prevost into geology,—"to explain facts which took place before the historical era by natural agents, which are still in operation."

The diluvialists, at least M. Durocher, use a different language. Existing currents, they say, do not produce effects which they are endeavouring to explain, and yet there were gigantic currents which produced them; thus, by their own confession, the greatest debacles, the most impetuous torrents, the most extensiue inundations, cannot reproduce, on a small scale, the effects of diluvian currents. Yet we know, that the most feeble rivulet shews us in miniature all the phenomena of a large river ; and everywhere in nature we see the same effects produced on every kind of scale. Our opponents confess that a torrent never traces rectilinear striæ; and yet they prefer to attribute them to torrents, rather than admit that they were engraved by glaciers, which are every day forming similar ones under our own eyes.

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* Calculations similar to those here given will be found in M. Martins' paper on the Glaciers of Chamonix, in our present number, p. 82; we have not, therefore, repeated them in this place.—*Edit. Edin. New Phil. Jour.*

These are the impossibilities which have prevented M. Durocher drawing any conclusion from the facts he mentions in his two memoirs. He endeavours to prove that these facts ought not to be attributed to the action of a glacier ; but he forgets to shew us how they can be explained with marvellous facility on the hypothesis of one or many currents. This however, he must do before he produce conviction in others. But the author has so completely lost view of the conditions of the hypothesis he has undertaken to defend, that his objections to glaciers apply almost always with still greater force to the supposition of one or more currents.

I have not adverted to many of these objections, from the fear of extending this refutation too far, and prolonging a discussion, which I leave in the hands of all geologists who have seen existing glaciers, and compared their effects with those observed in the plains of Switzerland and Scandinavia.

On the Theory of Dew. By M. MELLONI ; in Two Letters to M. ARAGO.*

The violent attacks lately made on the theory of Wells, have induced me again to take up the study of dew. After a very long series of observations and experiments, often interrupted and resumed, I think that I have arrived at a distinct solution of all the questions connected with this interesting phenomenon. The memoir in which they are described has been read to our Academy of Sciences (at Naples) who have been so attentive as to occupy themselves with the subject t three successive sittings. I have sent you an extract, with a request that you would communicate it to the Institute. You will see, my illustrious friend, that *there was something to be done;* but the observers who have made such severe attacks on Wells' principle were instigated by a spirit so blindly hostile, that, far from seeking for any thing that might be wanting, they wished to overturn and destroy all, to reintroduce (who would believe it?) the old phantom of the rising of dew from the earth!

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* Comptes Rendus des Seances de l'Academie des Sciences, No. xiii., p. 531.

After Wells' experiments, we may well admit with perfect safety, that dew does not rise from the earth, that neither does it fall from the sky, and that it is formed by the elastic and invisible vapour diffused throughout the space which surrounds bodies; and it is thus that we have comprehended all, by attributing, with the natural philosopher just named, the precipitation of the aqueous vapour to the cold resulting from the calorific radiation of bodies towards a clear sky. According to this manner of viewing the subject, the leaves of vegetables, wood, glass, varnish, *lamp-black*, become covered with dew, because they allow the heat to escape readily, and are considerably cooled under a clear sky; while metals remain dry on account of the difficulty they have in vibrating their heat towards the higher regions of the atmosphere. And, in truth, we observe a great difference between the indications of a thermoscopic apparatus, when we present to it successively a vessel of polished metal full of boiling water, and another vessel exactly similar, having its sides covered with varnish or *lamp-black*, the second action being much more energetic than the first. The inference is a just one; but it must be confessed that it may well be that it should not appear necessary or inevitable in the eyes of every one. In fact, Benedict Prevost, and Saussure before him, ascribed the want of dew on metals to an electrical influence. Leslie explained this phenomenon by a particular repulsion which metallic surfaces exert towards aqueous vapour; and the actual partisans of the disputed theory of the rising of dew, account for it by the heat and electricity disengaged in the chemical action of metals on the molecules of this same vapour at the moment of their passage into a liquid state.

To shew that these different hypotheses cannot be maintained, I first took three thermometers graduated on the stalk; I then passed each tube through a cork stopper, and fixed it at 5 or 6 millimetres from the bulb. This stopper served as a point of support to two parts of the metallic apparatus, in which I enclosed my thermometers intended for experiments on nocturnal cold. The first is a small vessel of silver or copper, very thin, resembling a thimble, and having its surface smooth and polished, and of sufficient size to contain the bulb of the instrument. The second is composed of a cylinder of tin, closed at one extremity and open at the other, forming an envelope to a graduated tube. These two metallic pieces (which can be taken off and put on with the greatest ease) are easily kept in their place by the pressure and elasticity of the cork.

Now imagine three wide recipients of tin, having a lateral opening by which we may introduce, close to the bottom, the armed bulbs of three thermometers, leaving their stalks and envelopes disposed horizontally on the outside; imagine these recipients supported by slender tubes of metal, provided with covers of the same substance, the whole exposed to the free air in a calm and clear night; and remembering that one of the thermometer's armature is blackened, the two others in their natural state, and the recipients sometimes open and sometimes closed,—you will have an idea of the experiments I instituted to compare the nocturnal radiation of silver with that of *lamp-black*. Let us suppose the recipients to be first closed; our three thermometers then indicate the same temperature. Let us then leave one of the vessels containing the metallic thermometers closed, and open the two others. Very delicate instruments and very minute comparisons are requisite to observe and measure the extremely slight sinking produced in the metallic thermometer exposed to the sky; but the blackened one will visibly sink, and, after some minutes, will indicate three or four degrees less than the thermometer in the closed vessel,—an evident proof that this difference is owing to the calorific radiation which the blackened apparatus vibrates to the sky, and not at all to the contact of the external air, which takes place equally on it and on the polished metal apparatus of the other exposed thermometer.

My memoir contains the detail of all the precautions which it is necessary to observe, in order to obtain the comparative degrees of cold produced by the radiation of silver and lamp-*black*. The definitive results have confirmed, in a striking manner, the fact lately announced to the Academy by MM. de la Provostaye and Desains, namely, that the emissive power of metals is much less than has hitherto been believed, according to the experiments of Leslie, Dulong, and Petit.*

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* The radiation of lamp-black being equal to 100, laminated silver would radiate, according to my experiments, about 3·026. MM. de la Provostaye and Desains found 5·37 for silver precipitated chemically on copper, and 2·10 when this precipitate is polished with the burnisher. According to them, the emissive power of silver just from the rollers is 2·94; and 2·38 that of laminated and burnished silver.

Observations made in 1838 led me to this result, that the difference of radiating force manifested in Leslie's famous experiment on the two faces of a cube, the one smooth and polished, the other rendered more or less rough with grooves, does not arise, as was generally believed at the time, from a variation in the mechanical state of the two surfaces, but from a change in the degree of density resulting from the operation, by means of which the smooth surface was transformed into a rough one. This proposition appears to me to be perfectly established by the three following facts: 1. The variation of emissive power produced by grooves does not appear much except in the metals; marble, jet, and ivory, grooved or polished, always radiate with the same energy. 2. Silver melted, and cooled slowly in moulds of sand, polished with oil and charcoal, and then grooved with a diamond in such a manner as to compress and condense the bottom of the rays, diminishes instead of augmenting its radiating force in passing from the polished to the rough state. 3. This same kind of melted and polished silver becomes much less radiating when beaten on an anvil, or passed under the *laminoir*.

It is easy to see that the experiments of the skilful natural philosophers above mentioned, furnished results altogether analogous, and, consequently, shew the same thing; for silver precipitated chemically on copper, being much less dense than laminated silver, and the latter being inferior in density to burnished silver, this latter property, according to the preceding numbers, is in the inverse sense of the corresponding emissive powers.

The only difference between the two demonstrations of the principle is, that my measures related to the most intense emissive power of silver, while those of MM. Provostaye and Desains referred to the emissive power of silver and other metals relative to the lamp-black.

It is, perhaps, to this unnoticed difference, or some other inaccurate data, Thermoscopic apparatus, similar to those I have described, having the protecting armatures covered with varnish, plumbago, glue, saw-dust, sand, earth, and leaves of plants, have invariably shewn a very sensible sinking of temperature before becoming moistened with dew; the interval has sometimes been many hours; often, also, there has been a lowering of temperature without precipitation of vapour at any period of the night. This latter phenomenon is produced the more frequently as the thermometers were placed at a greater distance from the ground. By operating, therefore, at a certain elevation, we may suppress or retard, at pleasure, the deposition of dew on bodies, and perfectly convince ourselves that it always follows, *never precedes*, the production of cold. With regard to the polished metallic armatures of my thermometers, I have never seen them covered with condensed aqueous vapour in very humid nights, when there was not a trace of mist in the atmosphere.

It thus appears that dew, properly so called, always requires a certain coldness in the body on which it gathers, and metals exposed to a clear sky are not covered with it, because they cool only in a very trifling degree. But may there not be other influences which prevent the precipitation

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that we ought to ascribe the historical error contained in the Notes presented by these gentlemen to the Academy. According to them, the relation hitherto admitted between the radiating powers of metals and of *lamp-black*, would result, not only from Leslie's experiments, but also from mine, and from that of Petit and Dulong. It is true that Dulong and Petit have found numbers differing but little from the relations ascribed by Leslie to the radiating powers of metals and *lamp-black*. With regard to myself, I am not occupied with researches of this nature. The only questions on calorific radiation, considered as to its origin, which appeared to me to require further illustration by means of the experiment so as to deserve for an instant the attention of natural philosophers, were, the above mentioned action of the asperities of the surface of a warm body, and the action of colour, both solved in the negative; then the influence which the thickness of the superficial bed, from which the interior radiations issued, exercises on the intensity of the radiation; an influence which appears to me sufficient to account for the enormous difference which exists between the emissive power of metals and that of other bodies. With regard to the value of these two powers, and their numerical relation as resulting from my experiments, any one may readily satisfy himself that these were never taken into consideration in my memoirs.

or accumulation of dew on metals? In other words, is the feeble radiation of metals the only and true cause on account of which these substances are never moistened with dew?

The following experiment appears to me to decide this question completely, and it demonstrates, at the same time, the erroneousness of the hypothesis of the rising or falling of dew, as well as the truth of Wells' principle.

On a disc of tin, of a single piece as broad and thin as possible, I traced a concentric circle of a radius equal to one-third of that of the disc, and covered it with a thick coat of varnish. I then cut another sheet of tinned iron, a second disc of 10 millimetres narrower than the varnished circle; and having soldered to its centre, and perpendicularly to its surface, the extremity of a wire 2 millimetres thick, and from 2 to 3 decimetres long, terminating in a point at the free extremity, I perforated the large disc with a small central opening, and introduced the wire into it on the side of the varnished surface. The moveable piece was then pushed along this wire till its distance from the small disc was reduced to about 5 millimetres; it was then fixed in this position by means of some drops of solder.

The two plates, thus united into a single system, are conveyed in the evening to the middle of the fields, and left for a few minutes to themselves in a horizontal position, and far from the contact of any other body. If the night be calm and clear, we then easily and distinctly see dew produced on the surface of the large disc.

It is sufficient, indeed, to remember that in the position we are supposing the two discs to occupy, the smallest is uppermost, and as its radius is 5 millimetres less than the varnished circle of the large inferior disc, it follows that an annular band of this circle, 5 millimetres broad, will stand out around the vertical projection of the roof formed over it by the small metallic disc above. It is clear that this band will radiate towards the sky, will diminish its temperature, and become covered with dew, and will by degrees propagate the cold and dew consecutively from the side to the centre, and from the side to the circumference. The propagation, however, will go much further in the latter case, for the points cooled by communication will become cold by radiation when they are covered with dew, while the varnished points placed under the small disc cannot well be cooled but by contact. The central part of the varnished circle continues always dry, and the metallic zone which surrounds it is moistened to the edge, if the atmosphere be excessively humid.

But what we would not expect at first sight is this,—the exact reproduction of the same appearances on the surface of the large disc turned towards the ground. The dew begins by shewing itself on this surface in the points opposite to the small annular exterior band of the varnish, and we perceive a slight whitish circle, which appears suddenly on the obscure field of the polished metal, reminding us of the daguerrian figures. This circle becomes stronger and dilates by degrees, sometimes reaches the edges, but never extends to the central part, which always preserves its dryness and metallic brilliancy, like the corresponding portion of the other side of the disc, and the small circular roof which covers without touching it.

This experiment, so simple, so economical, and so easy to be performed even by those who have never handled a philosophical instrument, is, so to express it, a speaking synopsis of the discussion of natural philosophers on the origin and nature of the nocturnal meteor of which we are now treating. Does dew fall from the sky? No, for the superior disc is always dry, and the largest part of the inferior disc moistened. Does it rise from the ground? Assuredly not, for if the middle part of the surface of the large disc turned towards the earth be covered with dew, there is always a dry and brilliant space towards the centre. Do metals repel the aqueous vapour which constitutes dew, or do they cause its evaporation as it is precipitated on their surface? Neither the one or the other, for you see metallic parts completely moistened, and other parts perfectly dry.

The first appearance of the dew on the band covered with varnish, and its successive propagation to the adjacent and opposite parts of the great disc, taken in connection with the sinking of the temperature which is seen in the varnished apparatus of the thermometers exposed to the air, finally proves that dew is a pure consequence of nocturnal radiation, which communicates to bodies possessed of great emissive power, the degree of cold necessary for the condensation of the elastic and invisible aqueous vapour diffused through the atmosphere.

Thus, all these facts perfectly accord with the theory of dew adopted by writers on natural philosophy and meteorology. I shall now mention other facts which cannot well be explained by this theory, but which connect themselves very happily with the principles of Wells, by means of considerations which I shall have the honour to submit to the judgment of the Academy.

I now proceed to develop (says M. Melloni in his second letter to M. Arago*) the proposition which I announced at the conclusion of my former communication, namely, that although we may regard the principles of Wells as to the origin of dew as unquestionably correct, it is impossible to give an account of all the thermometrical and hygrometrical phenonomena which takes place in the night, when the sky is clear and the atmosphere tranquil, unless we take into consideration a new circumstance hitherto entirely neglected, notwithstanding its extreme importance in the nocturnal cooling of bodies. But, in the first place, allow me to make a few observations on two series of experiments which the advocates of the rising of dew from the earth have put forward, with the allegation that they are sufficient of themselves to overturn all the theories of this meteor founded on calorific radiation.

They have taken a certain number of thermometers, equal, as far as possible, in size and sensibility. Some of them have been coated over with *lamp-black*, varnish, and china ink; others were gilded, silverized, covered with leaves of plants, of pewter, and of copper. Thus prepared, these instruments were exposed in the fields during a calm and clear night, and they indicated at first temperatures slightly different; but, after a certain interval of time, all of them at last nearly agreed. The experiment was varied in another manner. At the extremity of glass cylinders planted in the ground, they fixed horizontal plates of zinc, copper, and glass; each of these plates had a cavity in the centre in which the bulb of a thermometer was plunged, while the tube, supported by a wire, rose vertically above the superior surface; a thermometer, suspended freely between these plates, was intended to measure the temperature of the air. In this case, as in the former, the apparatus employed in experiment indicated, at the beginning of the night, calorific differences, which, at a later hour, disappeared; insomuch that, at the earliest dawn of the following morning, all the thermometers were found at heights sensibly equal.

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* Comptes Rendus des Seances de l'Academie des Sciences, tome xxiv., No. 15, p. 641.

These facts appeared to the opponents of Wells' theory, completely decisive; and they thenceforward maintained that "the pretended nocturnal cold of bodies, indispensable to the formation of dew, was a mere chimera!" For myself, I should affirm that if anything in this appears imaginary and fantastical, it will be found in the reasoning employed in drawing such an inference; for the experiments of these gentlemen were made near the ground, in an atmosphere charged with humidity. All the tubes of the thermometers were uncovered, and, in the last mentioned experiment, the bulbs of the thermometer communicated through the intermediate plates, with the cylinders which supported the apparatus. Now, the glass of which these tubes and cylinders were composed radiates considerably, its temperature sinks, and the cold thus acquired is communicated to the bodies that touch it: the latter being placed in a very humid air, then precipitate the aqueous vapour; but we know that water radiates and cools with as much energy as glass, varnish, and *lamp-black*. It was, therefore, nothing surprising that the thermometers in contact with the plates or plateaux of metal indicated, after some time, the same temperature as the thermometer surrounded with the most radiating substances. From the circumstance of the metallic surfaces, which were found covered with dew, becoming as cold as the vitrified or blackened surfaces, it might well follow that the water, glass, and *lamp-black*, are bodies possessed of emissive powers sensibly equal; but we could never be justified in inferring logically from this experiment, that metals cool, during calm and clear nights, as much as *lamp-black* and glass.

To learn the true state of things, it is necessary, therefore, to discard glass, and employ as substitutes, slender tubes of tin which scarcely radiate at all, and keep the thermometer sufficiently insulated from the heat of the ground; it is requisite, besides, to cover all points of the thermometer with metallic plates. Thus, these plates being polished, the thermometer gives as near as possible the temperature of the air; and when the covering is varnished, blackened, in contact with leaves of vegetables or any other substances, we obtain the degree of cold produced by the radiation of this substance, by a simple comparison with the polished thermometer.

By means of instruments thus mounted, I have convinced myself that the leaves of plants, glass, and varnish, and *lamp-black*, always cool, during calm and clear nights, from 1 to 2 degrees below the surrounding air.

On observing this slight diminution of temperature, we might be tempted to believe the decrease of 7 and 8 degrees, reported by Wilson and Wells, to be greatly exaggerated. But if we consider that the differences obtained by the two English natural philosophers contain an element foreign to the question: that the thermometers used for measuring the surrounding temperature were raised from 1^m 30, to 1^m 60, while the instruments enveloped in the radiating substance were placed near the ground, it is easy to understand to what the great difference between their results and mine are owing.

In fact, Pictet's experiments have long since demonstrated, that the temperature of the air decreases rapidly during calm and clear nights as we approach the surface of the earth. The single fact of this decrease ought therefore to render the temperature of the radiating substance placed near the ground inferior to that of the air, as indicated by a thermometer elevated from 1^m 30, to 1^m 60; so that, in an arrangement of this kind, the difference of the two instruments in no degree indicates the result sought for, namely, the cooling of bodies below the ambient medium.

There is, however, one experiment of Wells' in which a thermometer, wrapped about with wool, placed at the same level with a free thermometer, gave a lowering of temperature of 50°·3. Here the wool was cooled two or three times more than the *lamp-black* in my experiments; but I know that the emissive power of wool is not greater than that of *lamp-black*.

In order to determine the cause of this extraordinary cold observed by Wells, it was necessary, in the first place, to put the fact beyond doubt. With this view, having enveloped one of my thermometers in a tuft of wool, I exposed it along with two others of the same dimensions, one of which was covered with *lamp-black*, the other retained its metallic brilliancy, the instrument descended, in a few minutes, double the extent of the blackened thermometer. A fourth instrument, enveloped in an equal quantity of wool, condensed and pressed around the metallic cover by means of some rounds of thread of the same substance, gave an intermediate cold between the two preceding. I lastly prepared a fifth thermometer with a double covering of fine flannel, and it sunk still less than the fourth. I repeated these experiments by substituting cotton for wool, and obtained results in every respect analogous. I then perceived that the superiority of cotton or wool over *lamp-black*, in the phenomena of nocturnal refrigeration, was owing to a certain modification introduced into the emissive power of these bodies by the presence of air among their interstices.

But how does air increase the cold arising from radiation? The answer is simple and obvious. We have known, for many years, that the nocturnal coldness of bodies does not vary with the temperature of the atmosphere. Thus, Captains Parry and Scoresby have found, that during the calm and pure nights of the polar regions, the snow cooled about 9 degrees below a stratum of air raised from 1^m 30 to 1^m 60, when the atmosphere was from −25 or −30 degrees, and when its temperature is very near zero. M. Pauillet has seen swan-down descend to about 7° under the temperatures of

0° and of 25.° For my own part, I am convinced that blackened or varnished thermometers descend with a constant quantity, whatever be the temperature of the night. Now we may conceive that tufts of cotton or wool, placed on the upper part of thermometrical bulbs under the action of a clear sky, after having become cold by radiation, will communicate the cold they have acquired to the surrounding air, which, becoming heavier, will descend in the interior afterwards to fall to the ground; but this condensed air will always require a certain time to disengage itself from the obstacles which arrest it among the threads. The latter, therefore, will be surrounded with a colder air than at the beginning of the experiment; and, as their sinking of temperature below the ambient medium must continue invariable, it must necessarily happen that they will cool the more. This increase of cold will cause an additional depression of temperature in the medium; the latter, in its turn, will occasion an additional cooling in the radiating bodies, and so on consecutively, until the weight acquired by the condensed air release it from the obstacles which opposed its escape from the envelope.

What takes place with small quantities of cotton and wool artificially placed round thermometers, ought to occur naturally in many circumstances. In point of fact, plants with downy leaves are much colder than such as have smooth leaves. The temperature of grass and that of other low plants which cover fields, descends, in consequence of this frigorific reaction of the air, much below that of elevated bodies, on account of the neighbourhood of the ground which supports the ambient medium, and forces it to remain in the presence of radiating surfaces. In reality, the stratum of air in which meadow-grass is placed, is by no means immoveable; on the contrary, it whirls about exactly like the water in a vessel placed upon the fire; the aërial particles, condensed by the cold of the tops of the grass, descend to the surface of the meadows, are warmed by contact with the earth, ascend again towards the higher parts of the grass, and so on; but it is evident that, notwithstanding this state of agitation, they at last become colder, and that, in order to maintain itself constantly in a colder state than they, the grass must cool more and more, and this circumstance will induce a gradual refrigeration, and an increasing humidity in the stratum of air.

I do not here enter into the details necessary to shew how the principle of the frigorific reaction of the air serves to explain all the facts which precede and accompany the appearance of dew in meadows, and a multitude of phenomena of which a proper account could not hitherto be given. But your own discernment will supply this want; and I shall conclude by indicating the principal questions treated of in the memoir, which I shall soon have the honour to present to the Academy.

I may say, then, that besides the difficulties of which I have given a distinct solution in these two letters, my new experiments on the nocturnal cooling and on dew have enabled me to understand perfectly, 1st, The distribution of temperature among grass, which is found to be colder in the night among it than at the surface of the meadow. 2d, The inversion of the ordinary temperatures of the atmosphere near the earth's surface. 3d, The great humidity of the air near plants, from the first instant that the dew begins to appear. 4th, The injurious action of the least breath of wind. 5th, The formation and accumulation of dew during the whole course of the night. 6th, Its successive propagation from below upwards. 7th, The small quantity of dew on trees when compared with grass and low field plants. 8th, The disappearance of small drops of dew, which sometimes takes place on the lower parts of plants, while they are forming on the upper parts. 9th, The variable proportion of the meteor in the different seasons of the year. 10th, Its general distribution over the surface of the globe. 11th, The great difference between the diurnal and nocturnal temperatures of the torrid zone. 12th, The absence of dew in the small islands of Polynesia, and on vessels sailing in the midst of large seas. 13th, Its abundant formation when the vessels approach certain shores of continents. 14th, The sharp cold produced in the night in the sandy plains of central Africa. 15th, The natural and artificial congelation of shallow waters, when the temperature of the atmosphere is from 5° to 6° above zero, by taking into account the indisputable fact that water does not cool more than 1°·5 in consequence of its direct radiation.

I may finally add, that the part performed by stagnant air in the phenomena of nocturnal refrigeration, appears to me to have modified certain experimental data which have been founded on for calculating the temperature of space.

On the Relations which exist between the Phenomena of Erratic Blocks in Northern Europe and the Elevations of Scandinavia.

By M. DESOR.[*]

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There is a point in the phenomena of boulders not hitherto presented, which appears to me capable of throwing much light on the question that now engages our attention: it is the examination of the connection which subsists between the erratic blocks of Northern Europe and the elevation of upraising of Scandinavia. These relations are the more important, as they admirably explain some circumstances which are peculiar to the erratics of the north, and of which there is no example in Switzerland. These peculiarities are *1st*, the occurrence of polished and grooved surfaces beneath the present level of the sea; *2d*, the existence of *marine shells* attached to the polished rocks at a height much above the present level of the sea; *3d*, the presence of marine shells in the midst of the diluvium, even at an elevation of 800 feet; *4th*, the *osars*, or ridges of boulders and stones which contain the shells of the Baltic.

Among the phenomena which prove so fully the instability of the Scandinavian soil, there are some facts which indicate the rising of the land, while others, on the contrary, attest the subsidence. Thus we cannot have less equivocal proof of an elevation of a country, than the occurrence, at a great height, and at a considerable distance from the coast, of shells now inhabiting the adjacent seas, and whose perfect state of preservation leaves no doubt that they lived where they now occur; for, had they been transported by a current or any other violent agent, they would have been broken, or at least much worn. But, should we refuse to admit the evidence which these shells offer, we cannot deny the proof afforded by the serpulas of Christiania and the barnacles of Uddevalla, whose shells still adhere to the rocks far above the sea.

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[*] Communicated, by the Author through Prof. Agassiz, to Editor of the American Journal of Science and Arts, May 1847.

On the other hand, the fact that the striæ and furrows are continued *beneath the waters of the sea*, attests no less strongly that at a certain epoch the land must have been more elevated than now. In fact, it is a point on which the partizans of different hypotheses are nearly agreed, that the phenomena of erratics took place over a submerged country. Glaciers can advance only as far as the limits of the land. We learn, from the observations of M. Martins, that even the glaciers of Spitzbergen do not project beneath the sea; for, as the temperature of the water is above that of ice, it melts the glaciers by its contact, and a considerable space, equal to the height of the tide, separates the glacier from the water.*

But if, as I believe I have sufficiently proved, the polished surfaces of the north have been occasioned by immense glaciers, which have transported from afar the erratic blocks of Scandinavia, and furnished the materials of the diluvium and of the *osars*, it follows, that the whole country which bears traces of scratches, must have been out of water when the glaciers produced this polishing, and produced the striæ and furrows which we now see there. If these striæ were exactly at the level of the sea, we might suppose that Scandinavia was then at the same elevation as at the present day. But we have seen numerous cases in that island, in which the furrows are found *under* the sea; from which facts we must conclude, according to the principles laid down, that the land at that epoch was as much above its present height as the striæ are now below the waters. These results, although opposed, are not, as they may at first appear, contradictory; and it is here that the observation of shells completes the study of erratic phenomena, properly so called, by shewing us the chronological order of these events. In fact, the barnacles of Uddevalla, and the serpulas of Christiania, which are found, the former at the height of 200, and the latter of 170 feet above the sea, prove irresistibly that the coast has sunk in those places; the fact that these animals are adhering to striated rocks, shews not less certainly that the rocks were dry before these animals were living there; whence I read this double conclusion, 1*st*, that the graving of the rocks was anterior to the epoch of the barnacles and serpulas; and, 2*d*, that, to receive these animals, the coasts of Uddevalla and Christiania must have sunk as far, at least, as would be equivalent to the actual height of these fossils.

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* In order that glaciers should advance upon the bottom of the sea, it is necessary that the temperature of the water should be below zero the year through; but such a climate would render the formation of glaciers impossible. A humid atmosphere, rather than a severe temperature, is necessary to the existence of glaciers, and the former is incompatible with the temperature of a sea constantly below zero.

But the barnacles and serpulas are not the only proofs of this subsidence. At a much greater height we find shells, over these polished and striated rocks, imbedded in the diluvium; and as the species are in general indigenous, and probably contemporaneous with the serpulas and barnacles, it follows, of course, that the submergence must have been considerable, and equal, at least, to the site of the highest shells of the diluvium (800 feet). This submergence must, consequently, have taken place between the epoch of the furrowing and the stratification of the diluvium. At that time, the glaciers having quitted the plain to retire to the interior mountains, the waters of the sea invaded the low country of Scandinavia, surrounding the solid masonry of the Scandinavian mountains with an ocean to which we shall be able to fix some approximate limit when we are acquainted with the boundaries of the region of diluvial shells. In passing, I would remark, that the analogy of the erratic blocks of Finland with those of Scandinavia, allows us to believe that at that time the Gulf of Bothnia was not separated from the Arctic Sea.

We have no means of determining the length of time which elapsed between the retiring of the glaciers, and that subsiding of the land which led to the invasion of the sea. However *the perfect preservation* of the polishing beneath the diluvium, would seem to prove that the period could not have been long. Nowhere are the grooves and scratches more distinct, than where they have been uncovered by removing the diluvium; they usually form a striking contrast with those rounded rocks, whose surface has been for a long time exposed to the wearing effects of the atmosphere.* But as we have no reason to suppose that the action of the atmospheric agents was formerly less energetic than now, I am compelled to believe that the preservation of the polished surfaces under the diluvium, is owing to their having been exposed only a comparatively short time to wear from these agents. It is even probable that the invasion of the sea was one of the causes which have been active, if not decisive, in the destruction of the great glaciers. It explains, at the same time, the colder character of the diluvial fauna; for a mass of ice so considerable could not be melted without materially chilling the waters which bathed it. The cold having slowly disappeared, the temperature would have been gradually elevated, and the fauna of the waters would take by degrees the more temperate character which distinguishes it at the present day.

To this epoch of the invasion of Scandinavia by the waters of the sea, we should refer the arrangement in beds of the mud, sand, and gravel, which the great glacier has left in place, in testimony of its ancient extent. The action of waves coming in upon this moveable soil, has here overturned and heaped up the debris of marine life upon the shore, where the remains are found mingled with scratched rocks and pebbles. If such is really the origin of these deposits, there is no reason for surprise that the scratched pebbles should be so numerous. The waves, in striking them against each other, would wear them more or less; and if blocks of a considerable size have generally better preserved their markings than the pebbles, it is because, from their weight, they were less exposed to being moved and rolled. It is very natural in Switzerland, where the action of the waters has been less manifest and less prolonged, that the striated pebbles should be more numerous. Thus you do not find there, or only occasionally, distinct beds in the properly glacial deposits. Those which are met with, ordinarily occur in the neighbourhood of torrents.*

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* We may believe, that if, after a new deluge, the boulders which are at present exposed, should be covered again with gravel, the geologists of future ages would have much more trouble in ascertaining the causes which have produced the effects, than we have at the present day, aided as we are by the preservations, frequently admirable, of the polishing, furrows, and fine striæ.

After this epoch of immersion, even the proximate duration of which it is impossible at present to ascertain, the country of Scandinavia was again elevated. The shores bordering the high central regions, the plains of Sweden, and those of Finland, were successively raised from the bosom of the waters, bringing back with them to the surface the same mud, the same diluvial gravel, which had been deposited by the glaciers, and which had undergone no other change in the interval than that of being irregularly stratified and mingled with shells. The depressions of the land alone remained covered with water, and formed the lakes of Sweden and Finland, as well as the Gulf of Bothnia. The last, isolated from the ocean by the uprising of the intermediate land, has lost, by degrees, its saltness ; and this explains the character of its fauna, which is rather the fauna of brackish water than that of the sea. The interior lakes, also, were transformed completely into fresh water, and here and there may perhaps be found some indications of their ancient condition. It appears that certain fishes, in particular, have resisted these changes in the water ; and, according to researches of Scandinavian zoologists, especially of Mr Esmark, the trout of Swedish lakes (*Salmo trutta*, L.) is only a species of salmon, like the *Salmo salar*, L. But as the salmon of the coast does not ascend into the lakes, we are naturally led to the conclusion, that this fish has resisted the modifications that have occurred where it dwells. The emersion of land does not take place alike in all parts ; the beautiful observations of Messrs Keilhau and Bravais upon the ancient marks of the level of the Scandinavian sea, teach us that there it was not uniform throughout. Finally, if it is true that the *osars*, which may be traced a long distance into the interior, and whose mode of formation we have explained above, indicate successive sea-shores, it follows that the Gulf of Bothnia was once much larger than at present. A large belt of coast now under cultivation, was then under water, and only gradually became elevated.

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* Voy. Rod. Blanchet, Terrain erratique alluvien du Cassin du Leman.

This successive retreat of the waters has taken place mostly during the present epoch, as undeniable traces of man* are found in the interior of *osars*. It is probable that during this period of slow emersion, there lived in Scandinavia a primitive people, an entirely different race, as their osteology indicates, from that of recent Scandinavia, and whose skeletons are found in the peat beds mingled indiscriminately with those of animals, some of which have completely disappeared from the surface of the earth, such as the *Bos urus*, and others, which are no longer known in the same countries, as the rein-deer.†

Conclusion.—It follows, from the preceding considerations, that, whilst the upheavals of Scandinavia are of great importance in the study of erratic blocks, the latter furnish us, in their turn, with valuable hints as to the time and the geological bearing of these elevations. I have shewn that the elevations are not confined to the historical epoch, but extend far back into the period of the diluvium. On the other hand, we learn, from the same examinations, that these elevations have not been continuous; that, on the contrary, they were intermitting and in sweeps, since the land is successively raised and depressed. We have distinguished, in this connection, three principal points; 1*st*, an epoch when the land was higher than now, the epoch of the glaciers; 2*d*, a general sinking of the plains of Scandinavia, bringing with it an irruption of the sea; 3*d*, the rising of the same plains, which is even now in operation.

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* See Lyell upon the proofs of a gradual elevation of the soil in certain parts of Sweden.

† I am indebted to the beautiful work of Messrs Wilson and Eschricht for these details.

Each of these periods must have had a considerable duration. We can readily believe that, while an agent is acting so slowly that a glacier might transport, for hundreds of leagues, the stones and gravel taken from the Norwegian mountains, a long time must have been required, whose minimum of years would be thousands, if the movement of glaciers, in our day, be regarded as a criterion.

The second period must have been at least as long, if we consider the time required for the existence, propagation, and death of an entire fauna, whose numerous remains are found upon a land once submerged.

Finally, the third period comprehends the historical epoch, when the country was inhabited by the foreign race whose remains are discovered in the peat beds.

It follows, therefore, that the glacier epoch is not merely an accident in the history of our globe, but it embraces a long period, the more important to the geologist, that it is the connecting link between the antediluvian times and the historical era.

On the Meaning originally attached to the term "CAMBRIAN SYSTEM," *and on the evidence since obtained of its being geologically synonymous with the previously established term* "LOWER SILURIAN." By Sir RODERICK I. MURCHISON, G.C.St.S., V.P.G.S., F.R.S., &c.

["By such proofs (organic remains) we are enabled to distinguish the Silurian deposits from all others previously described, and through every lithological change we can thereby separate the System into Upper and Lower divisions."—*Silurian Syst.*, p. 9.]

In a communication upon the Silurian rocks of Sweden, published in the preceding number of the Quarterly Journal of the Geological Society, I stated my objections to some opinions of Professor Sedgwick, contained in the previous volume, which suggested a re-arrangement of the recognised divisions of the Upper and Lower rocks of the Silurian system. I was then chiefly called upon to point out the inapplicability of the proposal to remove the Wenlock formation (or a great part of it) from the Upper to the Lower Silurian; for even down to last year Professor Sedgwick had invariably spoken of all the lower palæozoic fossils of North Wales and Cumberland as belonging to one or other of those natural divisions.* In a word, the Upper and Lower Silurian fossils published by me had been appealed to by him as the types by which he worked out their equivalents in the above slaty tracts; and even in his memoir published in 1846, he still divided the older palæozoic rocks of our island into Upper Silurian, Lower Silurian, and Cambrian.† In the memoir, however, which he read at the last meeting of the Geological Society, Professor Sedgwick took a new course, for which I was unprepared by his previous publications.‡ After indicating the order of the strata in and beneath the Bala limestone, he now puts forth what I can only understand as this general proposition, viz., that although the fossil type of such rocks is essentially what he and others had recognised as "Lower Silurian," the term "Cambrian" ought in preference to be applied to them, because a succession of great physical masses exists in North and South Wales, which is not developed in the tracts formerly described in the Silurian region. In this way, the term Lower Silurian would be suppressed, and the Silurian system, deprived of its lower half would be split up into two systems of fossiliferous strata beneath the Devonian, instead of continuing to be one natural series divided into two groups, as first established in Britain, and subsequently extended to other parts of Europe.

Painful as it is to me to oppose my distinguished associate,—the friend with whom I have so often co-operated, and to whom I dedicated the "Silurian System," I am compelled to resist a proposition, the adoption of which would, I conceive, be productive of great disservice to the progress of geology; and he must therefore excuse me if I defend the ground of Caractacus, with a pertinacity approaching to that of the old Silurian chief.

I shall therefore treat this question under the following heads:—the origin of the terms "Silurian" and "Cambrian"—the nature and progress of the classification to which the term "Silurian" referred, whether in Great Britain or the continent,—and the effects of the adoption of the proposed change upon geological science.

After labouring during four years in certain Welsh and English counties, in which I had traced out and defined a succession of fossiliferous deposits, from the base of the old red sandstone down to certain older rocks then called "slaty greywacke," I was urged by many geologists to designate such rocks by a distinct name. Accordingly in June 1835,‡ I applied to them the term "Silurian System," thereby to distinguish a series of strata several thousand feet thick, and having a general community of fossil characters within the ancient British kingdom of the Silures. These I divided into two groups, the Upper consisting of the Ludlow and Wenlock, the Lower of the Caradoc and Llandeilo formations; the chief or typical fossils of each, being identified or named by Mr James Sowerby and myself, were then enumerated in tables.*

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* See Quarterly Journal Geol. Soc., vol. i., p. 1; No. 4, p. 442; vol. ii., p. 106.

† Ibid., vol. ii., p. 130.

‡ Let no one suppose, that in consequence of the divergence of our present views on a point of geological nomenclature, there has been the slightest cessation of friendly intercourse between us. Professor Sedgwick wrote to me four months indeed before his memoir was read, and candidly explained his opinions.

‡ Phil. Mag., June 1835, vol. vii., p. 50.

At that time the word "Cambrian" was unknown, and it originated in the following manner.† In a published diagram of succession, the Upper and Lower Silurian rocks were represented as reposing on *unconformable* strata which were termed "slaty greywacke," which was meant to represent certain inferior unfossiliferous rocks (like those of the Longmynd range of Shropshire). Now, on sending a copy of my new classification to M. Elie de Beaumont, that eminent geologist, wishing to mark strata separated by lines of dislocation by separate names, suggested the propriety of further distinguishing those last-mentioned unconformable and inferior rocks by the term "Hercynian," as taken from the Hartz mountains in Germany, where, as he then believed, the oldest slaty group would prove to be of higher antiquity than the strata to which I had applied the word "Silurian." Unwilling that the name for these infra-Silurian rocks should be taken from a foreign country, in which no precise palæozoic horizon had then been fixed, I at once urged Professor Sedgwick to apply to his slaty rocks, which were confidently believed to be inferior to my own, some term, on the same geographical principle by which I had been governed in proposing "Silurian." I even ventured to suggest the word "Snowdonian," because I knew that my friend then considered the NW. portion of the Welsh chain to be made up of the oldest fossiliferous masses; but preferring a more comprehensive geographical name, he took that of

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* Even at that early period I guarded against the possible inference that the mineral masses so defined were to be viewed as persistent. I shewed, for example, that in some tracts the lower division exhibited great thickness of sandstone beneath flags and limestone; in others *vice versa*. The same was explained at greater length in the work subsequently published (1839), wherein the evanescence of mineral types being indicated, it was maintained that the fossils alone could truly indicate the age of the strata. This fact is now mentioned, because, notwithstanding all my caution, it has recently been stated, that flagstone beds, with *Asaphus Buchii* occasionally lie above sandstone to which the word "Caradoc" may be applied. My classification, I repeat, was never based on such mineral succession; for whether the lower rocks were flaglike and calcareous, sandy or quartzose, schistose or slaty; whether the uppermost bed was sandstone and the lowest schist, or the reverse; their age was alone to be determined *by their imbedded fossils*. And as to the mineral base, I will cite my own words:—"Although the calcareous flags of Llandeilo, with their accompanying schists are considered to form the base of the Silurian system, their place is sometimes taken, *often, indeed, they are underlaid*, by sandstones of considerable thickness."—(Sil. System, p. 8.)

† See also Sil. Syst., p. 8.

"Cambrian." With this arrangement we both felt certain, t
anomaly could be introduced into the lower palæozoic classifi
as the relations and fossil contents of mineral masses whicl
contiguous must be eventually cleared up, without fear of en
the introduction of theoretical views.

The word "Cambrian" (as far as I know) was first used ir
by myself in the year 1836, in describing the structure of Pem
shire. But whilst I then spoke generally of such Cambrian
and afterwards at greater length in my large volume, their ai
and examination formed no part of my scope; that task havin
specially undertaken by Professor Sedgwick, at a time when I
believed, that from their great thickness, apparent inferiori
different lithological structure, they would be found to contain
of organic remains distinct from those I had called "Lower
rian." Improving in the following years the description of th
rian region, and having completed as well as I could by my o
bours (aided by a few kind friends resident in the country), tl
lection of its typical fossils, I published in 1839 the "Siluria
tem," as founded on my own researches. This work was
panied by a map, which, as far as the parts coloured Silurian ar
cerned, has, I believe, been found little fault with; the distir
of the Upper and Lower Silurian rocks, with their subordina
mations (all perfectly conformable, be it observed, to each
being accurately laid down in reference to other overlying de
But notwithstanding the labour bestowed upon it, this map w
cessarily very defective in the boundary-line between the t
Lower Silurian, and the *un*typified Cambrian rocks. In the no
part of the map that line was, indeed, for the most part insert
my request, by Professor Sedgwick himself;* and in Centr
South Wales it indicated, according to my view, little more tha
to the west of it, the so-called Cambrian rocks were then r
zoologically nor physically explored—a task which, as repeated
nounced to the Geological Society, and expressly declared i
work, was left exclusively to Professor Sedgwick. But altho
was, so to speak, a point of honour with me not to trench up
privileges of the eminent geologist who had engaged to descri
Cambrian region, I can truly say, that I repeatedly urged h
determine the organic remains of those slaty rocks; for I
thought, I repeat, that they would be found to be in that m
essentially distinguished from my Silurian types. Years, ho
rolled on, and this desirable end was not accomplished; a
peculiar fossils having to this day been published, the Can
system consequently remained without organic characters of its
It was, in fact, a great physical mass without the vitality
system.

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* See description of the map, Sil. Syst., p. xxix.

All that I knew of the fossils of the co-called Cambrian rocks, at the period of the publication of my work was, that the chief forms which occurred in their Eastern or upper division, as at Bala, were specifically the same as my Lower Silurian types,—a strong proof that there was a gradual transition from the one to the other, and that the boundary-line drawn upon my map between the tracts named Siluria and Cambria was merely provisional, and had been laid down on no fixed geological principle. Thus, in speaking of some of these Bala fossils, my words were,—" As these shells abound also in the Lower Silurian rocks, it would seem that as yet no defined line of zoological division occurs between the Lower Silurian and Upper Cambrian group; *and that as our knowledge extends, we may probably fix the lowest limit of the Silurian beneath the line of demarcation which has for the present been assumed* "[*] Surely words could not more explicitly shew, both that the boundary-line marked in the map was not a natural limit, but simply a geographical (not a true geological) line which ran between my working-ground and that of Professor Sedgwick; whilst they clearly indicated my belief, that the Silurian system, as distinguished by its fossils, would probably be carried further downwards when the region should have been thoroughly explored.

In fact, the problem to be then solved was—whether the enormous masses of rock which seemed to rise out from *beneath* the Bala beds would be found to contain a distinct group of organic life? If not, I for one should never have thought of considering them as constituting a system equivalent to the Silurian.

In the mean time, other geologists began to occupy themselves in North and South Wales. Mr Bowman made an excellent addition to our acquaintance with the former, by shewing the exact equivalents of some of my Upper Silurian strata in Denbighshire; and whilst Professor Sedgwick was renewing his labours, Mr Sharpe also entered the Welsh arena, and contributed, as the Society knows, many new and valuable data. But whoever was the inquirer, no fossils were reported or heard of but " Silurian." In South Wales, Sir H. de la Beche and the Government Surveyors shewed, that the rocks of certain quartzose and slaty tracts which had been loosely mapped as Cambrian by myself, were, in truth, physically, as well as zoologically, the same Lower Silurian strata I had described on their eastern and southern frontier, and that thus, with the exception of certain limited districts occupied by very ancient unconformable greywacke void of fossils, as at St David's, &c. (like that of the Longmynd in Shropshire), the lowest fossiliferous strata in South Wales were all conformable and connected masses. Even in a geographical sense, therefore, the question whether the so-called Cambrian rocks could be characterised by peculiar fossils, seemed to be thus narrowed to the space included between Cader Idris, the Menai Straits and Bala.

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[*] Sil. Syst., vol. i., p. 308.

Having satisfied myself in 1842, in company with Count Keyserling, that the prominent fossils in the rocks of Snowdon were nothing but published species of Lower Silurian Orthidæ with simple plaits, I next learnt from Professor Sedgwick himself, that he could discover no types differing from the Lower Silurian in any tracts of North Wales or Cumberland. Referring, indeed, to his works, as well as to those of Mr D. Sharpe, it was evident that whatever differences of opinion might exist between those authors concerning points of structure, the arrangement of the masses or their geographical boundaries, they both agreed as to the persistence of known and published Silurian types through many of the lower slaty strata of North Wales and the Lake Country. Thus fortified by the assurances of my contemporaries, including the Surveyors of the Government, and by personal visits to North Wales, Westmoreland, and Cumberland, I ventured to comment on the facts in my Address to the Geological Society in 1843, broadly stating, that as the whole of the *fossiliferous* series of North Wales seemed to exhibit no vestiges of animal life different from those of the Lower Silurian group, the tract must henceforward be considered of that age. It was on this principle that I then coloured a geological map of England, at the request of the Society for the Diffusion of Useful Knowledge.

In the mean time, however, though, for the reason before assigned, I had taken little part in the local question of what might be termed Cambrian, *as distinguished* from Lower Silurian (leaving that point to be finally settled by the Government geological surveyors), I had, by more extensive surveys in other parts of Europe, gathered positive evidences for coming to a definite conclusion respecting the base of the Lower Silurian rocks. Finding that in Russia, the lowest fossiliferous band was unquestionably my Lower Silurian group* (a view which was unanswerably sustained by the inquiries of my associates De Verneuil and Von Keyserling), I further saw this group in Scandinavia (with nothing but fucoids in its lowest beds), resting at once on pre-existing slaty and crystalline rocks. Northern Europe was first distinctly appealed to as exhibiting the base of the series of animal life, and the Lower Silurian was, by observations in Scandinavia, thus shewn to be the protozoic group. Then came the comparison of the lowest fossiliferous strata of North America, by American authors and Mr Lyell, with a Lower Silurian type similarly related to pre-existing rocks; and, lastly, in the first chapters on the geology of Russia in Europe and the Ural Mountains, the grounds were fully explained (1845) on which the conclusion had been generally arrived at, that the Lower Silurian was the protozoic or oldest type of animal life yet discovered.

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* See announcement to the York Meeting of the British Association, 1844.

None of my opinions successively put forth from the year 1843 to the present time were objected to, and I naturally therefore believed, judging indeed also from Professor Sedgwick's new map of North Wales,* that the question of palæozoic nomenclature was settled. I firmly thought, that however thick, however diversified in mineral characters all Welsh and British, as well as all foreign strata, in which certain typical forms prevailed, would be included in the Lower Silurian group. From what, I inferred, could it import in a nomenclature founded on the principle of "strata identified by their fossils," whether the rocks termed Lower Silurian were two or three thousand feet thick in the region where they were first described, or many times as thick in the western parts of North Wales, provided they were in both tracts characterised by the same groups of fossils? I knew, for example, that in the very thin but undisturbed bands of Russia, a greater number of species of animals had been found than had then been detected on the same parallel in the greatly expanded North Welsh rocks. It therefore seemed to me to matter little to the geologist occupied with the history of successive races and their apparition on and disappearance from the surface, that he should be told of a tract, in which there was a much grander and more diversified mineral character (with enormous porphyritic masses) than in the region wherein he had previously described similar *zoological* types.

The names of William Smith remained unchanged, although his original mineral. types were in aftertimes found to be often of very small persistence. It was not proposed to change the name of Lias, when that formation was found to be three times as thick at Whitby (with numberless new inferior strata and many new fossils), as in the South of England, where it was first named. Modern geology will stand on an insecure basis, if the principle of identifying strata by their fossils be abandoned. But it is now proposed to abrogate the established name of the type, and substitute for it that of a tract, the fossils of which have never been published, long after the Lower Silurian rocks were first named and distinguished by peculiar forms —when the fossils of that age have been so labelled in numerous museums on both sides of the Atlantic, and when geologists and palæontologists of all countries have adopted the term, and already know (through the previous admissions indeed of Professor Sedgwick himself), that the greatest portion of the Cambrian is zoologically nothing more than a downward extension of the Lower Silurian group.

Let me then ask my brother-labourers to adhere to the use of a name which has been so long current, and which for some years has had a definite meaning attached to it, both by its fossils and position in various parts of the world. Geologists have already honoured me with their approbation, for having worked out certain phenomena, which explained the first clear succession downwards from the base of the old red sandstone. They will not, I trust, forget the toil by which these results were obtained, nor cease to be alive to the fact, that, by the forms which I described, order was at length elaborated amidst various slaty tracts, often highly dislocated, and in parts metamorphosed, over which such Silurian types are *now* found to spread. For, even in reference to Britain, the succession of the broken and porphyritic region of North Wales, Westmoreland, and Cumberland, might, I apprehend, have long remained undeciphered, if it had not been for the constant appeals which the geologists who have explored those tracts have made to the established Silurian strata. Is then the key which has served to open out such regions to be now thrown away?

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* See Geological Journal, vol. i., p. 1, and map.

But passing for a moment to some of its leading details, let us now see if the memoir recently read by Professor Sedgwick contains anything new to lead us to change the previous arrangements. Enlarging the discovery of Mr Davis announced to us in 1845,* of the existence of a species of Lingula in the rocks near Tremadoc in Carnarvonshire, Professor Sedgwick tells us, that this band lies many thousand feet beneath the Bala limestone, including, however, it will be observed, enormous interpolations of stratified igneous rocks.

I grant that his sections (obscure as he admits some of them to be) afford proof of an enormously thick succession of "great physical masses." They clearly indicate, that in the early days of submarine life, the area of the earth's surface now constituting Merioneth and Cærnarvonshire, was much agitated by plutonic eruptions which threw down thick sheets of porphyry, trappean ashes, and other igneously formed matter on the bottom of the then sea, and that in relation to them, the quiet sedimentary deposits containing animal life were small. By such operations, and by the subsequent eruption of other igneous rocks, the whole series was, in these tracts, greatly expanded and diversified, whilst by other events the masses were thrown up into lofty mountains, and underwent much crystallization. But then comes the question, what are the animals which lived during the accumulation of this extravasated and troubled marine series?

Assuming that Professor Sedgwick be perfectly correct in his interpretation of what are really the lowest strata (though as yet we have no proofs like those derived from Scandinavia, that the *hypozoic* rocks of Anglesea are inferior to the lowest fossiliferous beds), we are informed by him, that in passing from W. to E. on several parallels, whether near Tremadoc, near Penrhyn, or again near Cader Idris, there exists an ascending series from lower slaty and quartzose strata through igneous rocks in which no fossils have been observed, and thence into beds charged with a Lingula and fucoids; and that after passing through other alternations of slaty and igneous rocks, the geologist reaches the chief fossiliferous zone of the north-western portion of Wales, as exhibited in the range of Snowdon.

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* Quart. Journ. Geol. Soc., vol. ii., p. 70.

He also points out that the same beds with Lingula occur along the Merioneth anticlinal in the valley of Festiniog, and that in proceeding westwards therefrom, a grand succession is seen up to the well-known horizon of Bala. Admitting that the Lingula of these low beds in North Wales is a new species, that single circumstance cannot surely be of much value in a discussion like this. Lingulæ occur in nearly all the formations or subdivisions of the Silurian system of Britain, from the Ludlow rocks to the Llandeilo flags, whilst in North America, as in North Wales, they are found at the base of the whole of the same series of animal life. Again, in Russia, where fucoids only have been detected in the lowest strata, the shells immediately above them are Orbiculæ and Ungulites (Obolus), both of them, like the Lingula, small horny bivalves suited to a sandy or muddy sea-bottom in which there had been little calcareous matter. In North Wales, as in other palæozoic countries, the true test of the age of the rocks lies in the lowest zones in which the common and characteristic fossils are found. Now, among the lowest of these North Welsh beds, Professor Sedgwick mentions the *Asaphus Powisii*, a crustacean first described by myself from the very uppermost beds of the Caradoc sandstone of Shropshire, or rather from the Horderley limestone at the base of the Wenlock shale, its associated fossils near Tremadoc, being *Graptolites Murchisonæ* and *G. foliaceus*, both common Silurian forms. A Homalonotus is next cited as pertaining to still higher beds, and is identified with a species from the Lower Silurian beds of Wittingslow in Shropshire.

The *Leptæna sericea*, a species frequent in the Wenlock shale, and which also ranges, as I have shewn, throughout the Lower Silurian rocks, is said by Professor Sedgwick to run low down into the Snowdonian group, together with the *Trinucleus Caractaci*, the most typical perhaps of all the Caradoc fossils; whilst the associated Orthidæ are nearly all the very same simple-plaited species figured in my work, and on which I have always dwelt as the best and surest types of the Lower Silurian strata; viz., *Orthis calligramma*, *O. vespertilio*, *O. flabellulum*, *O. Actoniæ*, *O. expansa*, *O. elegantula*, *O. Pecten*, &c.

Let these species be found in any part of the world, and I ask if their discoverer can possibly assign to them any other geological name than Lower Silurian fossils?

And here I would say a few words on one of the propositions of my friend Professor Sedgwick. Changing the old name "Lower Silurian" into the new one of "Cambro-Silurian," and shewing that there is a considerable intermixture of Upper and Lower Silurian forms in the group, he states with justice, that it should never have been cut off from the Cambrian. But why was it so cut off?—simply, I repeat, because the zoological contents of the great mass of the

Cambrian rocks were unknown at the period when the original classification was proposed. As soon as this point was cleared up, I hold that the conduct I pursued, so far from being in antagonism with the rules and analogies which have hitherto guided geologists, was in direct obedience to the only canon on which their nomenclature has been based,—viz., conformity of succession, and similar organic remains. The only error committed, was the original one of giving a systematic name to a mass of rocks before its fossils were known; and I venture to declare it to have been not merely my opinion, but that of every geologist who considered the subject, that the continuance of the recognition of a Cambrian system has been considered to be exclusively dependent on the discovery in it of a peculiar type of *life*, distinct from that formerly described as Silurian.

In respect to the geographical propriety of language on which my friend insists, I have already said, that I was not responsible for the outlines and contents of his Cambrian region; and as to the observation about the Silurian system being now brought by increase of knowledge to mean nothing but "fossiliferous greywacke," I have simply to remind him, that until the Silurian system was fixed, and was followed by tracing the ascending succession of palæozoic life through the Devonian into the Carboniferous and Permian deposits, foreign geologists had indiscriminately applied the word "greywacke" to different members of this great palæozoic series. But I need not dwell on the advantages which followed from that first step in the palæozoic classification, as they have been kindly admitted by my contemporaries, including Professor Sedgwick himself.

Although it is unnecessary that I should deny, what I apprehend no modern geologist can sustain, that peculiar lithological features or extraordinary thickness can constitute any claim for the establishment of a new nomenclature, I may be excused for requesting that reference be now made to my former descriptions of such Lower Silurian strata, when I found them intermingled with rocks of igneous origin in the Silurian region. I then specially described districts in the higher and western parts of Shropshire and in Radnorshire, in which contemporaneous submarine volcanic rocks alternated frequently with beds containing Lower Silurian fossils; such masses having been subsequently penetrated by other eruptive matter. Those contemporaneous trap-rocks were stated to be made up of many varieties, including felspathic grits, sandstones, conglomerates, and breccias, greenstone and horneblendic rocks of many shades, with porphyries, &c. Now, although these rocks had a very different mineral aspect from that with which I was familiar in districts removed from such disturbing causes, I never thought of applying a separate name to them. They had, it is true, a peculiar aspect. They were often much swelled out by the interlacement of porphyries, greenstones, and "volcanic grit," and owing to these conditions, fossils were comparatively rare; but in deciding their age I appealed to the fossils only, and wherever I found certain forms, of Orthidæ, Trilobites, Graptolites, &c., I at once mapped in such masses as "Lower Silurian." As no one has ever doubted that the name was there rightly applied, I request geologists to read the chapters descriptive of these rocks,* and then to tell me if I have uot in them given a fair illustration (though on a smaller scale) of the leading features which are said to characterise the infinitely grander masses of North Wales. With the exception of beds charged with Lingulæ and pisolitic iron-ore, and marked by a perfect slaty cleavage, there is, I assert, no essential difference, whether mineral or zoological, between the above tracts of Shropshire and Radnorshire, and the rocks of Cærnarvon and Merioneth; and if the hundreds of feet of the one be expanded into the thousands of feet of the other, and the undulating hills ranging from 1200 to 1800 feet high, be raised into rocky ridges from 2000 to 3000 feet high, geologists will have before them, in my opinion, the Cambrian system as now characterised by Professor Sedgwick.

Nature's legends are, in a word, found to be composed of the same fossil types in the western parts of Wales, as in the western limits of the Silurian region; the only difference being, that in the former the spaces between the letters are vastly more expanded, and that the whole region is more slaty, igneous, and crystalline.

I say it advisedly, and after consultation with good palæontologists, who have examined the North Welsh fossils, that there is no essential difference between them and those of my Lower Silurian tracts. On the other hand, there are considerable variations in the distribution of the Upper Silurian species of the region I first described, and those of parts of Wales, Cumberland, and Westmoreland, which have been recognised by geologists, including Professor Sedgwick himself as Upper Silurian. Why then does he speak of and apply my Upper Silurian types only throughout his last memoir, and not equally depend on those styled Lower Silurian? Why, indeed, is the term "Lower Silurian" not once employed in his last memoir, though abundantly referred to in all his previous communications, when my types were appealed to? The only answer, it seems to me, which can be given, is, that as the word Cambrian is now demonstrated to be zoologically synonymous with Lower Silurian, Professor Sedgwick will not abandon the name he formerly applied to his great physical group, though such name was used before its fossil contents were known.

The question, then, is simply, Will geologists find it possible to use two terms to designate the very same succession of animal life upon the surrace of the globe?—such terms being relatively made to depend upon the greater or less thickness of the strata and their diversity of lithological structure?

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* See Silurian System, chapters xxii. and xxvi.

If, then, greater thickness of the masses be abandoned as a reason for a separate name, some geologists might contend that a physical separation of the upper and lower groups, or the unconformability of the one to the other, would afford grounds for such a distinction. But even this feature is wanting, in reference to the two groups characterised respectively by Upper and Lower Silurian fossils. There are, indeed, districts in which one portion of the Upper Silurian group is unconformable to another; and, again, lines of dislocation producing unconformity occasionally affect the subordinate members of the lowest group itself, which Professor Sedgwick recognises as one natural whole. But we already know from the survey of the Government geologists, that all the Silurian strata roll over in conformable folds throughout South Wales, and that there is no general break between the masses occupied by Upper and Lower Silurian fossils in Wales, any more than within the limits of the Silurian region first chosen as a pattern. Indeed, I have strong grounds for believing, that the very rocks of Bala, about which so much discussion has taken place, will prove to be the physical equivalents of the schists, flags, limestones, and sandstones which, in South Wales, have been described by me as Llandeilo flags.

There being, then, no unconformity between my Silurian and the Cambrian of Professor Sedgwick, the only remaining ground for changing the name, is the opinion which he seems to entertain, that the Silurian system, as originally described, is in reality made up of two natural history groups, and ought therefore to have two names. On this point also, it is scarcely necessary that I should go beyond the clear evidences recently afforded by the Professor himself, of the great interchange of fossils between the Upper and Lower Silurian groups,* to convince every one that they are so knit together in Britain, as to be geologically inseparable.† When I published the 'Silurian System,' I then knew that a limited number of species only passed from the upper to the lower group, but in succeeding years I learnt, that many more were common to the two groups, both in Great Britain and in other countries. My last memoir on Gothland has shewn, that out of 74 species of shells and crustacea in the Upper Silurian rocks of that island (46 of which are British Upper Silurian forms), 9 at least range into the Lower Silurian rocks of Britain, whilst 14 is the number, if the Lower Silurian type of Northern

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* See also Quarterly Journal Geol. Soc., *ante*, vol. ii.

† The researches of the Government Geological Surveyors will presently bring to light facts which will place beyond all doubt, that even in North Wales or Cambria itself, many Upper Silurian forms are intermixed with those of Lower Silurian age; in short, Professor Edward Forbes has assured me, that in North Wales there is but one natural history system only.

Europe be included. On the plain fact, therefore; that there are many species of Trilobites, Orthidæ, and other shells which unite the two groups, I maintain that the Lower Silurian cannot be viewed as a system independent of the Upper.

But it is not on the duration or passage of species from the one group to the other, that I alone depend for the conservation of the zoological unity of my system. The qualifications and character of what I term a system are chiefly based on the assemblage of its classes of animals. Thus, the Silurian was typified as the great system of Trilobites, which crustaceans rapidly dwindle away in the overlying Devonian, and expire in the Carboniferous system. Again, the Silurian system was represented as being the chief centre of Orthidæ, its lower half being specially marked by small species of that genus with simple plaits. It was further spoken of as charged with Graptolites, and also as being the horizon in which certain very peculiar chambered shells are most rife. Of late years its lower strata have been shewn to abound in Cystidea, those simple forms which, chiefly by the labours of Von Buch, have been shewn to be the earliest created forms of the great family of Crinoids. And here I would beg British geologists to attend to the importance of foreign comparisons, if they wish to see rock systems founded on laws of general distribution of animals. Abounding in the Lower Silurian rocks of Scandinavia and Russia, these Cystidea had not been found by myself in the Lower Silurian rocks of Britain, but the researches of the Government Geological Surveyors detected the common species of Northern Europe (*Echino-sphærites aurantium*) in strata actually described and coloured in the map of the Silurian region by myself as Llandeilo flags, whilst the same observers are now detecting the same in greater quantity in the rocks of Bala and in Ireland.

Even whilst I write, I learn that the only strong distinction which was thought to exist between the Upper and Lower Silurian rocks has vanished by the discovery of the defences of cartilaginous fishes of the genus *Onchus* in the latter, as just announced by Professor Sedgwick; and thus, whilst my view of a period void of vertebrata, founded though it was on very general observation, must be abandoned,* I naturally rejoice in this unexpected additional evidence, whereby the Upper and Lower Silurian rocks are still more firmly united in one system.†

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* See Russia in Europe, and the Ural Mountains.

† I also learn from Professor E. Forbes, and the geologists of the Government Survey, that they have detected the defence of an Onchus in the limestone near Bala. Professor Sedgwick states, that the species he mentions were found in the Upper Llandeilo flags. Professor Phillips has detected fish-remains in the Wenlock shale, and they had been previously observed by the Rev. C. Brodie in the Wenlock limestone. Although the species of North Wales are not yet described, it is rather remarkable that the Onchus of these Lower Silurian rocks is said to resemble, to a great extent, the *O. Murchisoni*, Agassiz, of my Ludlow rocks.

Whilst we are considering what are the natural distinctions of a system, or "terrain," in the sense in which I used the word, I beg to be permitted, without further reference to Russia and Scandinavia, to shew how, in another portion of the continent, the Silurian system has recently been applied by a French geologist, M. Barrande, to Bohemia, the country of his adopted residence. The slaty fossiliferous rocks of Germany, like those of Devon and Cornwall are, it is well known, for the most part of Devonian age; but in Bohemia, a long and wide tract consists of a basin of Silurian rocks, the lowest strata of which repose on sedimentary rocks void of fossils, and these again on crystalline schists. With no other guide than my original work, the "Silurian System," M. Barrande, after collecting 600 species of Bohemia fossils, 129 of which species are Trilobites, has, of his own accord, come to the conclusion, that the whole clearly indicate a true Silurian series. The lower half of this series is composed of two stages of quartzose and argillaceous strata, which are not merely referred to the Lower Silurian as a whole, but, through their mineral characters and their Trilobites, and other organic forms, are even severally compared with the Llandeilo flags and Caradoc sandstone. The upper group, eminently calcareous, presents itself in three stages, the lowest of which M. Barrande (after most assiduous examination of the fossils) compares with the Wenlock limestone and shale, the middle with the Lower Ludlow rock (the chambered shells of which strikingly resemble those of the same formation in England), and the third or upper division with the Aymestry limestone and Upper Ludlow rock. I advert, therefore, with pleasure to such labours, because they prove that the detailed descriptions of certain typical Silurian tracts in England have not been unfruitful, even in reference to other and distant European tracts. I further trust that geologists may regard my late memoir on Gothland as a corroboration quite as striking, of the value of the detailed original description of the types of my own country.* On the other hand, I well know that there are Silurian tracts in the British Isles, which, under different conditions of mineral origin present much fewer points of resemblance to the types of the Silurian region than the above mentioned distant foreign regions.†

But rock systems must not be so formed as to suit one country only. For if it be granted, that in Britain the Lower can generally be so separated from the Upper Silurian, as to be capable of demarcation on a map, how could the geologist succeed in separating the Upper and Lower Silurian of Christiania in Norway, where these groups, each equally well characterised by fossils as in our own country, fold over together in such small united masses ?*

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* See Quarterly Journal Geol. Soc., vol. iii., p. 1.

† Whilst these pages are passing through the press, I have learnt from Mr M'Coy, that several of the Upper and Lower Silurian species of Ireland, which he has described and named. are identical with specimens from Bohemia now in the collection of the University of Cambridge; and together, with this fact, it is curious to remark, that the mineral characters of the rock masses in the two countries coincide; and that in Ireland, as in Bohemia, the Upper Silurian is eminently calcareous, the Lower Silurian sandy, quartzose, and slaty. (See the Silurian fossils of Ireland, 4to, as published by Mr Griffith and Mr M'Coy). —*9th April* 1847.

Having stated that the Upper and Lower Silurian rocks of Europe, including all the fossiliferous strata of Wales, are so knit together by fossils and the transitions of the masses, that they must be viewed as one natural history series, I will now conclude by simply indicating the small area in various countries to which the Silurian system would be reduced, if the meaning of that term were to be changed and restricted to the upper half of the original system.

In England, though prominent in the typical districts of Shropshire and Herefordshire, the Silurian rocks, so dismembered, would occupy a mere band (scarcely to be defined on a general map) in Brecknockshire, Carmarthenshire, and Pembrokeshire; whilst all the broad and specially typical Lower Silurian tracts laid down by me in Salop, Montgomery, Radnor, and South Wales, must be erased, since it is impossible to distinguish them by their organic remains from the groups of Snowdown. In Ireland, from what we already know, whether through the works of Mr Griffith, Captain Portlock, or the labourers of the Government Surveyors, the system would pretty nearly disappear, as the great mass of the Irish lower palæozoic fossils are found to be Lower Silurian.

In Russia in Europe, and in nearly all Scandinavia, Lower Silurian rocks and fossils only prevailing, the very name Silurian would be swept from the map, and the system so attenuated, would there be confined to Gothland and some small Baltic isles!

Now, it must be borne in mind, that even when the lower and upper groups are united in one system as at present, the Silurian rocks of Russia do not occupy one-fifth part of the area of either the Devonian or Carboniferous systems of those regions,† whilst in Germany, the whole system, as at present united, bears an infinitely small proportion to the overlying Devonian group. A glance at the Geological Map of America, in Mr Lyell's work, shews to what a small area, in relation to the other palæozoic rock system, the Silurian would be reduced, if its lower half were abstracted.

Independent, therefore, of the impropriety of mutilating a system established on the community of its zoological contents, the results of such an arrangement would be a violation of the meaning which ought in fairness to be attached to a great natural history period, which was typified as such before the Devonian System was thought of. There are indeed authors who think, that the Upper Silurian is so linked on to the Devonian, that the former, or a large portion of it, might advantageously be merged in the latter; and if their views prevailed, the only portion of my system or terrain which Professor Sedgwick's proposition leaves to me would also be swallowed up, and thus, by invasions on both sides, the poor Silurian system would be obliterated.

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* See woodcut, Russia in Europe, &c., vol. i., p. 17, and Quarterly Journal Geol. Society, vol. ii., part 2 (Miscell.), p. 71.

† See General Map, Russia in Europe, and the Ural Mountains.

On the principle, however, of ***strata identified by their fossils***, geologists, I hope, agree with me to the conviction in which I abide, that in whatever rocks, and to whatever depths the Lower Silurian types extend, the tracts so characterised must be considered to belong to the "Silurian System."

In contending for the preservation of that system in its unity, I in no way detract from the very great merit of the researches of my friend, Professor Sedgwick, in developing the physical structure, dislocations, slaty impress, fossil characters, and other phenomena of the rocks of North Wales, Cumberland, and Westmoreland.

As already stated, I formerly hoped he would also point out in their lowest division the existence of a distinct zoological type which would have entitled those rocks to a separate name; but having failed to do so, it seems to me manifest, that his "Cambrian" cannot now be sustained by dismembering a fossiliferous system which has been so long established, so largely developed, and so widely applied over the world as the Silurian. Whether geologists will use the word "Cambrian," in reference to still older and often unconformable greywacke lying beneath all the beds with Silurian fossils, it is not for me to determine. My chief object in this communication is to explain how, by the progress of research, the protozoic types of various parts of Northern Europe, including North Wales, have been shewn to be true equivalents of the lower part of a natural system which I proposed twelve years ago, and which geologists of various countries adopted after a careful scrutiny of the evidences on which it was established.

On the Protein Question. By Professor MULDER of Utrecht. Communicated by Professor J. F. W. JOHNSTON.

DURHAM, 17*th June* 1847.

MY DEAR SIR,—Will you favour me by inserting in your ensuing number, the following additional communication from Professor Mulder on the Protein question? The perusal of it will shew you that we are slowly progressing towards a more perfect knowledge of this important substance.

The state of our knowledge in regard to it is at present nearly as follows:—

1. There exists in the parts of animals and plants a substance to

which the name of protein is given, and which, according to the analyses hitherto made, may be most nearly represented by the empirical formula,—

$$C^{36}H^{25}N^{4}O^{10} + 2\,HO.$$

The older formula, now abandoned by Mulder, was—

$$C^{40}H^{31}N^{5}O^{12}.$$

The latter of these formulæ contains a larger proportion of nitrogen than the one now adopted. This arises from the circumstance that a part of the nitrogen supposed formerly to belong to the protein is now found to exist in it in a different state (that of an amid), and not properly to belong to the composition of the pure protein itself.

2. That this protein combines with the amid of the hypo-sulphorous, and probably of the hypo-phosphorous acid, and forms combinations which occur abundantly in nature. Albumen, casein, fibrin, &c., are such natural combinations. The nitrogen of these amids was included in the old formula for protein, and hence, when it is abstracted, the new formula must contain less nitrogen.

3. That these combinations of protein with the amids, when heated upon a plate of silver, with a few drops of a solution of caustic potash, evolve ammonia and sulphuretted hydrogen; which last product blackens the surface of the polished silver.

4. That they also unite directly with the chlorous, the tannic, and other acids, forming combinations of the natural albumen, casein, &c., with these acids. Such a compound with albumen is formed when a solution of tannic acid is poured into an aqueous solution of the white of egg. These compounds all blacken silver under the influence of caustic potash, as the albumen and casein themselves do.

5. That by the prolonged action of caustic potash, aided by a gentle heat and access of air, the amids contained in these natural compounds are decomposed—their nitrogen more or less completely separated in the form of ammonia, and their sulphur converted into sulphurous and phosphorous acids which combine directly with the protein. When separated from the potash by acetic acid, the combination obtained contains these two acids in variable proportions, and does not blacken silver when treated as above described.

The prolonged action of chlorine produces a similar effect—forming at first compounds of the chlorous sulphurous and phosphorous acids with protein, which do not blacken silver.

6. But that by a prolonged action of chlorine under other known circumstances, the proportion of the sulphurous and phosphorous acids in these compounds diminishes, and becomes very variable. Mixtures in fact are obtained in which pure chlorite of protein exists contaminated with more or less of certain compounds of the same chlorite with the acids of sulphur and phosphorus.

Thus far at present—irrespective of personal contentions—our groping after the truth seems to carry us.—Believe me, yours very truly,

JAMES F. W. JOHNSTON.

To Professor JAMESON, Edinburgh.

UTRECHT, 27*th May* 1847.

MY DEAR FRIEND,—The albuminous bodies about which I wrote you some time ago, required a detailed investigation, in consequence of what I had discovered, namely, that the sulphur and phosphorus they contain, exists in the state of sulphamid and phosphamid, and that hyposulphurous acid is present in the precipitate produced by dissolving them in caustic potash, by the aid of heat, and adding acetic acid in excess.

I have, without intermission, continued my investigations, and the result is, that the above named constitution is common to them all. Hair, whalebone, casein, legumin, albumen, fibrin, horny matter and the nails, consist of nothing else than the above amids, combined in different proportions, either with protein, or with an organic group, which can bear no other name than that of oxide of protein. I formerly gave you instances of this in the composition of fibrin, horn, and legumin.

It is evident, after this discovery, that the empirical formula, by which we have hitherto represented protein, ought to receive some modification. All my former results tend to corroborate this, and you will find, that all that is necessary is a change of names, and that the results of my former analyses remain unaffected. It was wrong, therefore, to attempt to throw suspicion upon my results. Only, what I formerly called chlorite of protein, must receive the name of chlorite of albumen, of casein, and of fibrin, and if you change these names, then everything else remains the same. What I formerly distinguished by the name of sulpho-proteic acid, ought to be called sulphate of albumen, or of casein, but the analytical results stand unaltered.

It is only by my present investigations that I have become acquainted with chlorite and sulphate of protein. In composition and properties, they very much resemble the combinations of chlorous and sulphuric acid, with albumen, casein, &c.; but when heated with potash upon a silver plate, they do not produce a black spot. They—the chlorites, that is to say—all contain a small quantity of hyposulphurous acid; but, in some compounds, which otherwise are pure chlorites of protein, this hyposulphurous acid does not amount to more than 0·6 per cent. They thus prove, that this hyposulphurous acid, of which very variable quantities may also exist in protein, is not an essential ingredient of it; and, further, that notwithstanding all the trouble that is taken now to direct attention to this sulphur, it will be an unsuccessful attempt to deny the existence of an independent organic group, *which is free from sulphur in every sense of the word.*

If, for instance, a current of chlorine is passed through a solution of albumen, for several days together, we obtain not a product of decomposition, but a real combination of chlorous acid with protein, which does not blacken silver. The quantity of sulphur it contains, in the form of hyposulphurous acid, is equal to that of the sulphamid of an equivalent quantity of albumen. According to Kemp, this would amount to 1·5 per cent. in the chlorite compound; but this is too high, as I shall prove below. If duly prepared, it does not contain more than 1·2 of sulphur, equal to 1·83 per cent. of hyposulphurous acid.

But this proportion is far from being constant or essential; for if a current of chlorine is passed through the acetic acid solution, from which protein from fibrin has been precipitated, we obtain a chlorite of protein, perfectly similar to that from albumen, but containing only 0·4 per cent. of sulphur = 0·6 per cent. of hyposulphurous acid.

Similar discrepancies are found in the quantity of hyposulphurous acid, existing in protein itself.

After this sulphur question has produced its effect, it will descend into the place which it ought to occupy.

The maximum quantity of hyposulphurous acid which I have been able hitherto to find in, or to be taken up by protein, is 2·4 per cent., equal to 1·6 per cent. of sulphur—that is the quantity which is found in albumen, under the form of sulphamid. It represents exactly the half, in equivalents, of the quantity of hydrochloric acid, that can be taken up by albumen. This combination, therefore, cannot be called strange or singular. The hyposulphurous acid obstinately adheres to the protein in the same manner as sulphuric acid, phosphate of lime, metallic oxides, and several other bodies, all of which may unite with it in small quantities.

The composition of protein, free from sulphur in every form, may be represented by the following empirical formula:—

$$C^{36}\,H^{25}\,N^{4}\,O^{10} + 2\,H\,O.$$

It differs from the one to which I have hitherto adhered, because the organic group contains a little less nitrogen than was ascribed to it before. This is all the modification which my former results require.

The formula expresses in 100 parts.

C	54·7
H	6·8
N	14·2
O	24·3

The grounds upon which I propose this formula, are not more certain than those for all empirical formulæ. We have seen this in the case of salicine. All the empirical formulæ, which equally expressed the composition of salicine, fell to the ground after Piria had discovered its constitution. Yet the formula above given brings us a little nearer to the truth.

Perhaps it may be said,—And so every thing is modified that you have formerly stated regarding protein? On the contrary, nothing is modified but the formula. Science retains what it possessed, viz: that—

1*st*, The vegetable and animal albuminous bodies contain in common an organic group free from sulphur.

2*d*, There are two groups to be considered as higher degrees of oxidation of the former, and produced from protein.

I will give you the new form, into which my previous results require to be moulded. It will thence be evident to you, that with the exception of the nitrogen of the protein itself, nothing at all of my former results is altered.

Albumen from eggs, according to my former analyses,* and those of Rüling,† (C=75·12; N=87·5), and the new determinations of sulphur consists of

	M (1).	R (2).	Atoms.	Calculated.
C	53·5	53·4	36	53·8
H	7·0	7·0	26·85	6·7
N	15·5		4·42	15·6
O	22·0		12·	21·9
Ph	0·4		0·05	0·4
S	1·6	1·7	0·4	1·6

This composition represents a combination of 20 equiv. of protein + H O, with 8 equiv. of sulphamid and 1 equiv. of phosphamid. Kemp designates this explanation by the name of an artifice. This expression, however, is not accurate. It is nothing else than an attempt on my part, to give a certain form to what I have found. If the quantity of phosphamid in the combustion were double, it would be reduced to the simple form of 2 equiv. of protein + 1 equiv. of amid of sulphur and phosphorus.

Now, by the influence of chlorine, this albumen combines, without undergoing a change, with chlorous acid, and the product is chlorite of albumen, I.‡ which I before wrongly called chlorite of protein; I add, II.§ chlorite of casein, and, III.,|| chlorite of fibrin.

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* Scheik. Onderz. deel iii.

† Ann. der Chem. und Pharm., 1846.

‡ Bulletin, 1839, p. 400.

§ Ibid.

|| Ibid.

				Atoms.	Calculated.
I.	C	47·7	47·9	36·	47·2
	H	6·2	6·3	26·8	5·8
	N	13·9	13·9	4·4	13·5
	O			11·	19·2
	S			0·4	1·4
	ClO^3	11·7	11·2	1·	12·9

I have, for simplicity, left out the phosphorus; I do not even know if it exists in this compound.

II.	C	48·1	48·1	III. 47·9	48·0
	H	6·4	6·4	6·1	6·3
	N				
	O				
	S				
	ClO^3	12·3		11·6	

The quantity of sulphamid in fibrin and casein is different from that in albumen, but the difference is small, and therefore I pass it over here.

These bodies are the chlorites of albumen, fibrin, and casein. They produce sulphuret of silver, on being heated with potash upon silver, and thus they can be easily distinguished from the chlorite of protein containing hyposulphurous acid, which does not possess this property.

The latter may be prepared in different ways—among others, from the solutions, from which protein has been precipitated, or by passing chlorine, for a long time, through a mixture of albumen, fibrin, casein, horn, hair, whalebone, &c., and water. The product always contains a certain quantity of hyposulphurous acid arising from the decomposition of sulphamid and water $2 (S N H^2 + H O)$. The action of the chlorine upon this produces ammonia, which unites with hydrochloric acid,—and hyposulphurous acid, which, along with chlorous acid, is taken up by the organic compound.

In my former letter to you, I reduced the composition of protein with hyposulphurous acid, as it is obtained from albumen, to a group free from this mixture. I then brought into account 1·4 per cent. of sulphur, as was found by Fleitmann. The normal quantity obtained by fusion with soda, and subsequent addition of saltpetre, is 1·6 per cent., and this I believe to be the true proportion of sulphur in albumen. The compound consists of—

	Found.	Atoms.	Calculated.
C	53·7	36	53·4
H	7·0	27	6·7
N	14·2	4	13·9
O	22·7	12	23·7
$S^2 O^2$	2·4	0·2	2·3

This combination is present in the substance obtained by the long continued action of chlorine upon one of the above named substances. Only when prepared from albumen, it yields exactly the above proportion of hyposulphurous acid; but when obtained from the solution of fibrin, from which protein has been precipitated, it only contains 0·6 per cent. of sulphurous acid, and yet it consists of $C^{36} H^{27} N^4 O^{12}$, minus 1 or 2 equivalents of water, according as 1 or 2 equivalents of chlorous acid have been taken up into the combination.

I here subjoin the composition of chlorites of protein containing hyposulphurous acid, as they are obtained from horse-hair, human-hair, whalebone, and trioxide of protein, (Scheck. Onderz. deel i., p. 265; deel ii., p. 403), according to my former results:—

	Hair.		Whalebone.		
C	47·2	48·1	47·2	47·2	47·5
H	5·9	6·1	6·0	6·1	6·2
N		13·9			
O					
$S^2 O^2$					
$Cl O^3$	12·6	12·6			

Chloride of protein prepared from the trioxide (former results):—

					Atoms.	
C	48·2	47·2	47·5	47·1	36·	47·5
H	6·0	6·1	6·4	6·1	26·	5·7
N		12·9	12·8		4·	12·3
O					11·	19·3

When this compound, which is a bichlorite of protein, is dried at 212° F., it contains only 12·17 per cent. of chlorous acid.

The same body may be obtained from the solution of fibrin, from which protein has been precipitated, but this contains only 0·6 per cent. of hyposulphurous acid.

If through the solution of albumen, from which protein has been separated, a current of chlorine is passed, a combination is always obtained of $C^{36} H^{25} N^{4} O^{10}$ with chlorous and hyposulphurous acids, but in variable proportions. That of the latter acid varies, which apparently indicates that the sulphur is not inseparable from $C^{36} H^{25} N^{4} O^{10}$, and is no essential ingredient of protein. But the quantity of chlorous acid also varies, and the cause of this is, that, if the current of chlorine is continued for a sufficient length of time, we obtain mixtures of protein with one or two equivalents of chlorous acid.

I here give you the result of the analysis of these bodies, and this I do for two reasons:

1*st*, Because these very mixtures afford a proof that the combination contains chlorous acid, and not chlorine.

2*d*, Because they shew, that the sulphur they contain, is an accidental and not an essential ingredient.

		Relative number of Atoms.
C	45·4	36
H	5·6	26
N	11·9	4
O	18·2	11
$S^2 O^2$	1·1	
$Cl O^3$	17·8	

Another produced—

C	45·9	36
H	5·9	26
N	12·0	4
O	19·1	11
$S^2 O^2$	1·3	
$Cl O^3$	15·8	

A third yielded—

C	47·2	36
H	6·2	26
N	12·5	4
O		
$S^2 O^2$	1·5	
$Cl O^3$		

Here the proportion of sulphur is from 0·8 to 1·0 per cent., being, in the first instance, lessened to half the quantity contained in albumen.

Any one who can explain these results in another way, will do a service to science by making it known. I consider these bodies as mixtures of neutral and acid combinations of chlorous acid, with C $H^{25} N^4 O^{10}$, and a variable quantity of hyposulphurous acid.

All these chlorites dried over sulphuric acid at the ordinary temperature, appear white and pure. They have all the same properties, and tend to confirm the doctrine of protein.

As we must distinguish between chlorite of protein with hyposulphurous acid, and chlorite of albumen, fibrin, &c., we must do so also with their other combinations.

Thus, we must call the compound, which I have formerly analysed,* tannate of albumen. It consists of—

				Atoms.	Calculated.
C	54·2	53·4	53·6	54·0	53·9
H	5·6	5·7	5·5	33·8	5·6
N	10·5			4·4	10·2
O				22·	29·2
S				0·4	1·1

$=(C^{36} H^{25} N^4 O^{10} + (0{\cdot}4\ S N H^2) + H O) + (C^{18} H^5 O^9 + 2 H O)$; that is, albumen with tannic acid, which has exchanged one equivalent of water for one equivalent of albumen. You see, that this formula agrees also better than the former with my old results. The same applies to sulphuric acid with albumen, and sulphuric acid with protein.

When protein containing hyposulphurous acid is dissolved in strong sulphuric acid, the former is expelled and replaced by the latter, in a quantity amounting to 5 per cent.

In my following communications to you, I hope to treat of these and other combinations.

Believe me,
Yours faithfully,
(Signed) G. T. MULDER.

To Professor JOHNSTON.

Important Information regarding the Cultivation of Tea in the Valley of the Dhoon, India.

The last *Calcutta Gazette* contains a very important dispatch from the Court of Directors, relative to the capabilities of the Valley of the Dhoon, and the adjacent districts, for the production of tea. Several valuable reports have recently appeared in the local journals of the success which has attended the efforts of Dr William Jameson (nephew of Professor Jameson), Superintendant of the Botanic Gardens in the north-west Provinces of India, to introduce the cultivation of this plant in the north-west, and the public have, therefore, been in a measure prepared for the very gratifying intelligence now communicated to the community. But the report of the tea-brokers in England on the quality of the article submitted to their inspection, exceeds our most sanguine expectations. They have pronounced it equal to China tea of a superior class, possessing the flavour of the orange pekoe, but more than the usual strength of that tea, in other respects resembling that imported as Ning Yong. One of the gentlemen whose opinion has been obtained, declares, "that the tea shrub in Kemaoon is not only identical with the China plant, and as capable of being made into as fine a description of tea, but also that the soil and climate in Kemaoon are as suited to the favourable growth of the shrub as the finest of the China localities." The tea appears to be as much prized in the district in which it has been raised as it is in England, One hundred and seventy-three seers of it were recently sold at Almorah, and produced from four to five rupees the seer, which was equal to the price of the best tea sold in Calcutta, at the great tea depot of D. Wilson & Co. The price for which it can be raised, according to Dr Jameson's calculation, is so low as to afford the greatest encouragement to the application of capital. He estimates, that if cultivated on a sufficiently large scale, the prime cost in Calcutta, including every expense, would be little more than *eight* annas a seer, or one-eighth of the present price. Supposing, however, the cost of cultivation, manufacture, and transport, to be double this estimate, a sufficient margin of profit would still be left to gratify the most sanguine speculator. The capacity of the provinces of Kemaoon and Gurhwall for the enlarged production of the article, does not, moreover, appear to be limited to particular localities. According to the latest report which has been furnished to the Court, 176 acres were under cultivation, containing not fewer than 322,579 plants. The crop is thriving in different places over four degrees of latitude and three degrees of longitude, and 100,000 acres are now available in the Dhoon alone for the purposes of tea cultivation. At a maund an acre, they would yield 7,600,000 [illegible] which is equal to *one-sixth* the entire consumption of England. If the application of capital to this object receives sufficient impulse, we might, in the course of ten or twelve years, have the gratification of seeing England draw the largest portion of her supplies from this country, and a new trade of L.3,000,000 or L.4,000,000 sterling a year, grow up [illegible] India, stimulating industry and diffusing plenty, and increasing the mutual benefit which the two countries derive from each other. The multiplication of the supply in such abundance would also tend to reduce the prices at home and throughout the world, and thus augment the consumption. Nor must we forget the great impulse which these tea plantations—a large proportion of which would necessarily be fed by British capital—must give to steam communication between the two countries, and the additional importance they would impart to the establishment of a railway from the port of Calcutta to the north-west provinces.

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* Bulletin, 1839, p. 18.

But the country itself would reap no small advantage from the facility of obtaining supplies of tea at a reasonable rate. The natives of India are partial to this beverage, and the use of it will be extended in exact proportion to the degree in which it is brought within their means. A rupee and a half a seer, at which rate Dr Jameson calculates that the cultivation will yield a profit of nearly two hundred per cent., would place the Kemaoon tea within the reach of the whole body of the middling classes; and the demand for it, in the country itself, would be sufficient to furnish the highest encouragement to perseverance, independently of the European market.—*Friend of India, Serampore, Feb.* 4, 1847.

On the Occurrence of Stones on the surface of Glaciers, as connected with Glacier Stratification. In a Letter to Professor JAMESON by M. E. DESOR.

New Haven, 15*th May* 1847.

SIR,—I have just seen, in the fine library of M. Silliman, the 83d Number (vol. xlii.) of your Journal, containing two new letters upon glaciers by Mr James D. Forbes. The facts ported have so much the more interest in my eyes, as they agree, in a very satisfactory manner, with the observations made during the last year upon the glacier of the Aar, and reported in the new work of M. Agassiz upon the glaciers.*

It is easy to perceive, in reading over the most recent publications upon the glaciers, that the opposition between the different theories tends constantly to disappear, in as much as facts take the place of speculation; for there can be no doubt that the different physical laws, as gravitation, plasticity, expansibility, which have been insisted upon in a too exclusive manner by authors of different theories, have each some influence upon the mechanism of the glaciers.

Nevertheless, there are some points which are still viewed very differently in the different systems, and especially those concerning the structure of the glaciers. M. Agassiz, as you know, makes a distinction between the *strata*, which are the result of the annual deposit of snow, and the *blue bands*. The first, according to him, extend entirely through the glacier, and very often sand and fragments of rock are found between them. These are the so-called *dirt bands* of Mr Forbes. The blue bands, which are so frequent in the middle and lower part of the glacier, have not the same importance, being merely the result of superficial fissures parallel to each other, and usually agreeing with the general direction of the strata.

Mr Forbes, on the contrary, looks upon them as the consequence of the unequal rapidity of the movement in the different parts of the glacier. He mentions, in his 12th letter, a fact concerning the glacier of La Brenva, which, according to him, would be an "*experimentum crucis*" in favour of his theory. The glacier of La Brenva, in the valley of Chamouni, like most of the glaciers of the Alps, has increased very rapidly of late years. Mr Forbes has noticed that the blue bands are the most striking in the most advanced portion of the glacier, at the part below the promontory marked B in his figure (p. 100 of volume xlii. of this Journal). According to him, this is owing to a "longitudinal tearing," and the movement would be facilitated by the numerous fissures occasioned by this tearing.

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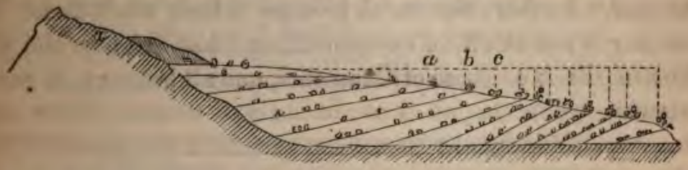
* Systeme Glaciare, ou Recherches sur les Glaciers actuels, &c., with an Atlas, 1847; not to be confounded with his former work, entitled, Etudes sur les Glaciers, 1840.

At any rate, I must say that this splitting supposes a degree of rigidity entirely at variance with the theory of semi-fluidity, and that it could hardly be accounted for, even by the theory of expansion. There is, I think, a much more simple way of explaining this phenomena; it is, that the glacier, after having passed the promontory, where it was somewhat compressed, comes out into a freer space, where it is able to expand, and the laminæ, being seen projecting one beyond the other, like the leaves of a book when bent, become more striking. The same thing takes place in the glacier of the Rhone, where the laminated structure is more striking below the fall than above; and similar effects may be observed in all the lateral expansions in the glacier of the Aar.

I come now to another point,—the appearance of stones on the surface of the glacier. It is a subject upon which most singular opinions have been expressed, and which appears, in fact, at first most enigmatical. When we examine attentively the debris on the surface of one of our large glaciers, we see frequently masses and tracks of stones, which commence in the very midst of the glacier, without any apparent connection with the mountain slopes from which the lateral and medial moraines proceed. We know, also, that the mountaineers pretend that the ice of the glacier is perfectly pure, including no kind of foreign body in its interior; and, in fact, it is true that we find very rarely any foreign body in the walls of the crevasses. Nevertheless, these are occasionally found; and therefore I agree entirely with Mr Forbes in thinking that all the stones lying on the surface of the glacier "must previously have been imbedded in the virgin ice." The question is, how did they reach the surface? Mr Forbes, referring to the glacier of the Rhone, which is nearly destitute of stones in the upper part of its course, whilst he says "they begin to appear at the surface at the terminal slope," has proposed a very unexpected explanation to account for their extrusion, which he has illustrated by the diagram, No. 83, Pl. II., fig. 7, of this Journal. He supposes that the stones "are actually introduced into the ice by friction at the bottom of the glacier, and forced upwards by the action of the frontal resistance which produces the frontal dip of the veined structure, and that they are finally dispersed on the surface by the melting of the ice."

There is scarcely any geologist conversant with the phenomena of the glaciers, who could be satisfied with this explanation. I shall endeavour to shew that the appearance of stones at the surface is obviously the consequence of the primitive stratification in the upper parts of the glacier.



Let the diagram be the longitudinal section of one of our large glaciers,—the Glacier du Rhone, for example. Like all the large alpine glaciers, its greatest thickness is probably at its beginning; but, in consequence of the annual melting, it becomes more and more reduced as it advances. In order to supply its place, a new stratum of snow is annually deposited at its origin, and in the mean time a certain quantity of rocks fall down on it from the neighbouring mountain slopes *x*. The next winter this layer, with its debris, will be covered by a new stratum; and if there were no melting, and if the motion were equal in all the parts of the glacier, this debris would remain buried (below the dotted horizontal line), and be for ever concealed in the interior of the glacier. But we know that a certain amount of ice is melted every year from the surface of the glacier, and in consequence of this, the debris become by and by discovered. The first will appear at the outcropping of the stratum *a*. Little farther below another boulder will come forth out of the layer *b*, another, again, out of the layer *c*, and so on, till they become so numerous that they form regular tracks. Beautiful examples of this appearance of stones and moraines are shewn in the map of the Glacier de l'Aar, in M. Agassiz' new work, to which I refer for more ample information on this subject. This appearance affords us, after all, the most striking evi-

dence that the primitive stratification is not obliterated, as Mr Forbes has maintained, but that it is preserved through the whole mass of the glacier. They shew also, better than any discussion, how little analogy there is between the structure of the glacier and that of a lava stream.

Barton, 30th May.

***Postscript.*—Concerning the motion of the glacier during winter, you will see that the results of Mr Forbes, as stated in his 14th letter, agree, in a very satisfactory manner, with those obtained by M. Agassiz and myself on the glacier of the Aar. In this respect, as in many others, the Glacier of the Aar is not at all an exceptional one, but is governed by the same laws as those of Chamouni, and all large glaciers in general.**

On the Manna of the Scriptures.

Dr Wright* describes a substance he met with, under the name of *manna*. We found, says he, "in one part of the mountains (probably on the return route) great quantities of a sweet substance found on the leaves of certain trees, generally the oak and gall-nut tree, and which is called *gezza* in Koordish, and *manna* in Syriac, and perhaps *honey-dew* in English. It forms on the leaves in such abundance, that, when they are dried and pounded, it comes off in scales, and is collected and used as an article of food. When melted and strained, in order to separate the crumbled leaves, it is very delicious, and is eaten by the people often in preference to honey. In the summer it is collected in large quantities, and put up for winter use. Often, as we were riding along among the trees loaded with it, we found it pleasant to break off the branches and lick the leaves, which were so coated with it, that in a very few minutes our appetites were satisfied. There is a species of willow growing in the water-courses in Persia, on which this article is sometimes found; but we have never seen it there in such abundance as in the Koordish mountains."

The term manna ordinarily refers to the well-known product of several species of Fraxinus, "that grow spontaneously in Italy and Sicily, and very probably in all the oriental Mediterranean region," and which is used for medicinal purposes. Substances which resemble this in form, or taste, or mode of production, are also called by the same term, which in every case, therefore, needs defining. The *gezza* was observed in June, and, from Dr Wright's account, we infer it was a semifluid exudation upon the leaves of certain trees.

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* Dr Wright is a member of the American Mission, at Oroomiah, in Persia.

This has been mentioned by writers in other parts of Asia; and as it differs so entirely from other forms of *manna*, which occur under peculiar circumstances, a few facts from various authors may be cited.

Wellsted,* in his journey from Tor to Mount Sinai, September 1836, "found in the Wadí Hibron, fifteen miles from the sea, and at an elevation of about two thousand feet, the tree which produces the *manna*. This remarkable substance is secreted by several trees, and in various countries in the East. In some parts of Persia, it is believed to be an insect secretion, and is collected from a shrub called *gavan*, about two feet high, bearing a striking resemblance to the broom. In the hilly district of Looristan (south-east from Koordistan), as in Mesopotamia, we find it on several species of oak, which there, however, are of more stunted growth than in England. From these the manna is collected in cloths spread beneath them at night, and it then bears the form of large crystal drops of dew, such as we see on the plants in England in the early part of the morning. Burckhardt observes, that at Erzrum a substance resembling manna in taste and consistence, distils from the tree which bears galls, and, with the inhabitants of the country, forms one of the principal articles of their food.

In the Horticulturist, No. 7, January 1847, is an article translated from the Revue Horticole, from which we extract the followlowing notice of what appears to be the same (or similar) thing with the *gezza*.

"Ehrenberg and Bové described the 'Manna Tamarisk,' which is abundant throughout Arabia, and even found on Sinai, 900 feet above the sea. The women and children collect this, which flowed from the branches of these trees. The Arabs clarify the manna by dissolving it in warm water, and making a syrup, the taste of which is equal to that of the best honey."

Under the word "Manna," in the Dict. Univ. d'Hort. Nat., Paris, 1846, is mentioned a short, stunted, spinous shrub, *Hedysarum alhagi*, Linn., *Alhagi maurorum*, Tourn., as growing in the deserts of Arabia and Persia, upon which is gathered a white concrete juice, which is called *manna alhagi*. Olivier, on his return from Turkey, brought to France several pounds of this substance, which, according to Niebuhr, is used in Persia instead of sugar in pastry, &c.

"Mr Lindley† has recently pointed out an oak, *Quercus mannifera*, from the leaves of which also drops a sweet substance, which seems to have been mentioned under the name *chelber* by Olivier. This name, which is applied by the hordes of Korassan and Little Tartary to a nutritious substance which falls on the ground, it is easy to see, approaches very nearly to that of *Semljenoi-chleb*, by which name the nations of the Kirghiz designate the *Lecanora esculenta*."

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* Travels in Arabia, vol. ii., pp. 47, 48.

† Révue Hort., as already cited.

Several occurrences of what is called a fall of *manna* are attributable to the accumulation of the lichen, *Lecanora esculenta Aucher-cloi*,* observed in Persia in layers of nearly inches (0 m. ·12 to 0 m. ·13) in thickness. He sent specimens, with the following note, to France:—"In 1829, during the war between Persia and Russia, there was a great famine in Oroomiah, south-west of the Caspian. One day, during a violent wind, the surface of the country was covered by a lichen, which fell from *heaven*. The sheep immediately attacked and devoured it eagerly, which suggested to the inhabitants the idea of reducing it to flour, and making bread of it, which was found to be good and nourishing. The country people affirm, that they had never seen this lichen before nor after this time." "During the siege of Herat (which is about 876 feet above the sea), more recently, the papers mentioned a hail of *manna* which fell upon the city, and served as food for the inhabitants." "A rain of manna occurred, April 1846, in the district of Jenischehir, and formed a layer three or four inches in thickness. It was of a greyish white colour, rather hard, and irregular in form, inodorous and insipid."

"Pallas observed it in the mountainous, arid, and calcareous portion of the great desert of Tartary. M. Eversham collected it in the steppe of the Kirghiz, to the north of the Caspian Sea, where it is called semljenoi-chleb. M. Ledebour has observed it in the same countries, but chiefly those which border on Altai and Bilezikdgi; saw it also in Anatolia, in 1845. Dr Leveillé gathered it in Crimea, and Dr Guyon recently in Algeria."

"It is found in irregular shaped bodies, varying in size from that of a pin's head to a pea or small nut; and when seen in its proper sites, has never been found attached to any support whatever. An analysis of the Lecanora shews that there is no fecula in its composition."

Wellsted, p. 49, "learned from a Jewish Rabbi, that, on his journey through the desert contiguous to Damascus, far removed from trees or vegetation of any kind, a substance was deposited, which, from his description, in appearance, size, and flavour, accurately resembled the manna of the Scriptures. Similar testimony was derived from several Bedouins."

It may be remarked, in passing, that several writers have not hesitated to identify some of these species with the manna miraculously supplied to the Israelites in the wilderness. They were obviously acquainted with manna of some kind, from the fact that they named the new substance from its resemblance to it.—*American Journal of Science and Arts, Second Series*, vol. ii., No. 9, p. 350.

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* Relat. d'un Voy. en Orient., vol. ii., p. 399.

On the Temperature of the Geyser Springs in Iceland.

M. Flourens communicated to the Academy of Sciences on the 16th of November, the results of some observations of MM. Descloizeaux and Bunsen last July, on the intermittent boiling springs of the Geyser and Strockr,* the latter being within 140 yards of the Great Geyser.† The observations were on the temperature of the water, in the great column or well of each, made by suspending thermometers at different depths, at different times, before and after eruptions. The Great Geyser has a depth of 22 metres (72 feet), and the experiments shewed that the temperature of the column diminished gradually from the bottom upwards, and that the maximum temperature at the bottom before a great eruption was 127°·6 Centigrade (260½° Fahr.), and the minimum 122° (251½° Fahr.), after an eruption. The temperature of the water at the surface was 85°·2 (185° Fahr.), when that at the bottom was 127° C.

After an eruption, the lowest thermometer stood at 121°·6 (251° Fahr.); nine hours afterwards at 123°·6 (254½° Fahr.). Between 11 o'clock A.M. of the 6th July, and 2·55 P.M. of the 7th, there was no eruption, so that there had been an interval of nearly 28 hours; and the water at the latter time, at the bottom, was 127°·6 (261½° Fahr.); a quarter of an hour afterwards there was a slight eruption.

The Strockr is a circular well 44½ feet deep, with an orifice of about 8 feet, which rapidly diminishes downward, and at about 27¼ feet from the surface the orifice is only 10¼ inches. The column of water between the eruptions has a mean depth of 27½ feet, so that its surface, which is in a constant state of ebullition, is generally from 10 to 13 feet below the surface of the ground. The temperature of the water at the bottom varied from 112°·9 to 114°·2 (235° to 237½° Fahr.), and the same temperature continued throughout a depth of about 20 feet, when it began to sink, and at the surface of the water the thermometer stood at 100° (212° Fahr.).

These observations on the temperature of the water are highly curious and important. We have a temperatnre of 261° Fahr. at the bottom of a free open column of water, in which thermometers could be suspended on a line dropped from the surface, while it might have been expected that, as soon as a film of water at the bottom was raised to a higher temperature, it would ascend, and be replaced by a colder and heavier film, and that thus a constant current would be established throughout the column, until the whole arrived at a temperature of 212°, when ebullition would commence and continue. The pressure of the column of water may perhaps account for the high temperature at the bottom, especially if the free circulation be impeded by the sides of the well not being vertical, and still more by projections in the sides causing contractions of its diameter. But the experiments of M. Donny, of the University of Ghent, published in the 17th volume of the Memoirs of the Royal Academy of Sciences and Belles Lettres of Brussels, on the Cohesion of Liquids, may perhaps be considered as throwing some light on this phænomenon of the Geyser. By a series of carefully conducted experiments, M. Donny has shewn :—

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* It is called Strokkus in the Comptes Rendus, but Henderson calls it Strockr, and says the name is derived from the verb "Strocka," to agitate, or bring into motion.

† Henderson's Iceland, p. 69.

1. That the constancy of the boiling point of water, under ordinary atmospheric pressure, depends upon its containing a considerable quantity of air.

2. That there is a marked difference between the boiling point of water containing air, and of water freed from air.

3. That a small quantity of air dissolved in water, is sufficient to attenuate greatly the *cohesion existing between the molecules of the water.*

4. That when water is freed from air, as far as that is possible, the cohesion of the molecules is so increased, that a higher temperature is necessary to overcome it, and that the boiling point is very considerably raised.

M. Donny succeeded in raising the temperature of water so freed of air to 135° Centigrade (equal to 275° of Fahr.), under the ordinary atmospheric pressure, without its exhibiting any symptom of ebullition,—shewing, that the cohesion of the molecules was nearly equal to the pressure of the three atmospheres on water containing air. This is a fact most important to bear in mind in reasoning upon many geological phænomena, particularly those connected with the solution of silica.

The further researches of M. Donny, recorded in the same memoir, appear also to offer an explanation of the violent and intermittent eruptions of the Geyser; for he states, that if water deprived of air be exposed to so considerable an increase of temperature as to overcome the force of the cohesion of the molecules, the production of vapour is so instantaneous and so considerable as to cause an explosion. As water long boiled becomes more and more deprived of its air, M. Donny attributes the sudden bursting of the boilers of steam-engines to the same cause.—*Address delivered at the Anniversary Meeting of the Geological Society of London. By Leonard Horner.*

On the Origin of the Sand-hillocks of St Ives Bay, Whitesand Bay, and Mount's Bay. By RICHARD EDMONDS, jun., Esq.*

The sand-hills of St Ives Bay, which are almost entirely covered with turf, the *arundo arenaria*, mosses, and other plants, occupy some square miles of the northern coast of Cornwall, and consist chiefly of comminuted marine shells, carried from the shore by violent winds.

They are called "the Towans" from the Cornish word *towyn*, "a turfy down,"—the word "down" being, perhaps, a mere corruption of "towyn" by the very common change of the letter *t* into *d*. And it is remarkable that the name *Les Landes*,† "barren heaths," given to the sandy district on the south-western coast of France, is almost precisely the same with *Lelant*, the parish in the Towans, where an ancient market-town is said to have been buried by the sand. Hence, *Towans*, *Downs*, *Lelant*, and *Les Landes* (here, again, the *t* is converted into *d*) may be all regarded as synonymous.

Some suppose, according to an old tradition, that these sand-hills, with the exception of a few feet immediately below the surface, were blown in during one tremendous tempest.‡

Others, judging from the dark horizontal lines—the remains of old vegetable surfaces—which occur at various depths within a few feet of the present surface, and which alternate with layers of light sand, consider that the hillocks were formed by a succession, at distant intervals, of thick deposits, which always buried the then growing turf, and sometimes to considerable depths.§

A third hypothesis which I would suggest is, that the sand has, for the most part, accumulated imperceptibly upon a continuously growing vegetable surface,—the deposits during a single storm being too slight to cover the herbage or to check its growth, except occasionally, when they were sufficiently copious to bury it entirely; yet not so deeply but that the turf has soon reappeared, and the gradual accumulation has proceeded as before. These occasional complete coverings of the herbage may be inferred from the dark lines or vegetable remains above noticed, and even in their absence from the perfectly preserved *land* shells which are frequently found more numerous in horizontal lines than in the sand immediately above or below them; for these lines, although not dark, are like the former, most probably, sites of old vegetable surfaces whereon the inhabitants of the shells subsisted. Generally, however, the shells are not thus unequally distributed, but occur precisely as if the sand had been gradually accumulating and burying them, without ever completely covering the growing turf whereon the animals were feeding or hybernating.

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* Read before the Royal Geological Society of Cornwall, on the 15th of October 1846.

† Lande "*grande etendue de terre qui n'est pas propre au labour.*" Dictionaire Breton.

‡ A similar opinion prevailed in reference to the sand-banks of Mount's Bay. See Trans. of the Royal Geological Society of Cornwall, 1826, p. 179.

§ See Sir H. De la Beche's Geological Report of Cornwall, Devon, and Somerset, p. 445.

A few months since, while examining some of the deep cuttings in the hillocks about a mile from the sea, I discovered, at the depth of about 50 feet, imbedded in a uniform mass of sand, a great number of small land-shells, within a space only half an inch thick and three inches square. The shells were mingled with sand; and as they consisted of not less than ten different species, the space, probably, small as it was, included several nests.

From the perfect preservation of these shells, and from the impossibility of their inhabitants finding subsistence on the bare sands, I conclude that they, as well as the numerous other land-shells which occur in an entire state in the hillocks at all depths, must have been imbedded *in situ*—in contact with vegetable surfaces, whose former existence, it appears therefore, is now generally indicated only by the exuviæ of the animals which once pastured on them.

The shells found in the small space above described, are of the following species, in various stages of growth:—*helix virgata*, and *pulchella*; *zonites radiatulus*; *zua lubrica*; *bulimus acutus*; *pupa umbilicata*, *marginata*, and *anglica*; *vertigo* *edentula*; and *bithinia ventricosa*. Those which I found in other parts of the Towans, at various depths, are *helix aspersa, nemoralis, hortensis virgata*, and *ericetorum*; *bulimus acutus*; *zua lubrica*; and *pupa marginata*; in all, fourteen species, of which the most abundant, as well beneath as on the present surface, were the *bulimus acutus*, *helix virgata*, and *helix ericetorum*.

Mr R. Q. Couch, author of the Cornish Fauna, who kindly furnished me with the names of these shells, makes the following interesting remarks in his letter to me on the occasion. "Of the *Helices* all are common in the locality except the *helix pulchella*. This last I have found near Falmouth, at Pendennis; near Penzance, at Trereife; and near the Land's-end. It is, however, rare in this county. The quantity found on the Towans within so small a space is very remarkable, and should further research shew that they are of frequent occurrence in other parts of the Towans, we must come to the conclusion that they were once abundant in Cornwall, but are now gradually becoming extinct in this locality. The *pupa marginata* and *bithinia ventricosa* are very rare: I have found a few dead specimens near Hayle, and at Whitesand Bay, which might have come from the sand-hills, but I have not yet found them alive in Cornwall. It would be very desirable to examine the Towans at different depths, to ascertain the changes which the Fauna of our county has been undergoing during the last one or two thousand years."

The observations now made on the origin of the sand-hills of St Ives, are equally applicable to those of Whitesand Bay,* near the Land's-end, and to the sand-banks of Mount's Bay, except that the last are principally granitic. I have examined the sand-bank between Penzance and Marazion, especially the highest part of it, near Marazion Bridge, and have found, throughout a depth of about 10 feet from the surface, a great number of land-shells (*helix virgata* and *bulimus acutus*) in perfect preservation, and in similar circumstances to those already described; so that (like the sand-hills of St Ives and Whitesand bays) these sand-banks have apparently arisen from successive deposits which did not cover the growing turf, except occasionally, when, being more copious than usual, they have completely buried it, although not so deeply as to prevent its speedy reappearance above the sand.

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* Besides the *helix virgata* and *bulimus acutus*, with which the sections of the sands of Whitesand Bay are thickly studded, I found there likewise a specimen of the *clausilia bidentata*.

To this process of accumulation by the agency of the winds, one very remarkable exception remains to be noticed. It occurs in the bank near Marazion Bridge, where, in one instance, a complete covering of the herbage appears to have proceeded directly from the sea—from the waves of an earthquake,* or some extraordinary high tide coinciding with a furious south wind. For in the section of this bank, a layer, an inch or two thick, of small rounded pebbles, may be seen 3 feet below the surface, and more than 15 feet above the level of high water; whilst in the subjacent sand, deposited probably by the winds alone, numerous perfect *land* shells are imbedded throughout a depth of 4 or 5 feet beneath the pebbles.

SCIENTIFIC INTELLIGENCE.

GEOLOGY AND MINERALOGY.

1. *Example of apparent Drift-Furrows dependent on Structure. By C. B. Adams, State Geologist of Vermont, &c. (Communicated for this Journal.)*—The attention of geologists having been lately called to the question, whether the grooves and striæ commonly attributed to drift agency, may not be due to *structure*,† it may not be improper, in anticipation of the results of the survey of Vermont, to mention an example in which this is undoubtedly the case. Mr Macintosh, the author of the article which is alluded to, and which was read before the Geological Society of London, particularly suggests that such may be the origin of the examples in the United States, described by President Hitchcock,—a suggestion, we will venture to add, which must have occasioned much surprise in those who are familiar with these effects of drift agency in the New England States, unless they also may have met with facts of the same nature with those which are the subject of this brief notice.

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* See Trans. of the Royal Geological Society of Cornwall, 1843, p. 113.

† In this Journal, 2d Ser., vol. i., p. 277; and Sir R. I. Murchison's Geology of Russia and the Ural Mountains, vol. i., p. 566.

Not far from the geographical centre of Vermont, in the town of Randolph, on ascending a hill a mile east of the centre village, may be seen a ledge of argillo-mica-slate, in which the planes of lamination do not coincide with those of deposit. The former have a direction of north 35° east (true meridian), and a dip of 30° to the north, 55° west. The true strata have a direction of north and south, with a dip of 65° west; and although much less conspicuous than the cleavage planes, are distinguished, without much difficulty, by a slight difference of materials, in consequence of which they weather unequally, so as to form shallow grooves with a well-rounded excavation of 3 to 6 inches wide, and an inch deep. The coincidence of the direction of these grooves, with the ordinary cases of drift-furrows, and their obliquity to the planes of lamination, which are obvious to the passing traveller, who may not, without special examination, recognise the true stratification of the rock, as well as the well-rounded excavation of the grooves, due, no doubt, to the gradual transition in the characters of the strata, combine to present a case of extraordinary resemblance to genuine drift-furrows.

It is proper to add, that the examination of several hundred examples of rounded, smoothed, striated, and furrowed rocks, has brought to light only this case, in which *structural grooves* bear any resemblance to those which have resulted solely from an external mechanical force. The suggestion of Mr Macintosh is, therefore, plainly incapable of general or even common application, although cases may occur in which it is worthy of careful attention. Indeed, President Hitchcock has himself* most scrupulously distinguished the drift-furrows from those which are due to structure. Sir R. T. Murchison also remarks, that "the greater number of the deviously parallel scratches on the worn surface of the hard crystalline rocks of the north, are, in our opinion, clearly *mechanical*, and cannot be connected with structural condition."—*American Journal of Science, Literature, and Arts*, vol. iii., p. 433.

2. *Food of the Mastodon.—(Proc. Bost. Nat. Hist. Soc.)*—Professor Gray stated, that there had been recently placed in his hands specimens of earthy matter, filled with finely broken fragments of branches of trees, which were said to have been found occupying the place of the stomach in the skeleton of the mastodon exhumed on Schooley's Mountain, N. I., and lately exhibited in Boston. As similar observations are said to have been made in several instances, Professor Gray was induced to examine the substance brought to him. The wood evidently consisted of branchlets of one, two, and three years old, broken, quite uniformly, into bits of half an inch or so in length, with only now and then traces of the bark remaining on the wood. The wood was not at all fossilized, and was but slightly decayed. From the appearance of the branchlets examined, Professor

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* Final Report on the Geology of Massachusetts, p. 385.

Gray inferred that they belonged to some coniferous tree or shrub, and probably to a kind of spruce fir, rather than to a true pine. This inference was borne out by the examination of thin slices of the wood by the microscope. The woody fibre was very beautifully and distinctly marked with the circular discs that are characteristic of all coniferous wood. The structure agreed quite perfectly with that in similar branchlets of the common hemlock spruce.—*American Journal of Science, Literature, and Arts, New Series,* vol. iii., No. 9, p. 436.

3. *Large Plates of Mica.*—The Vittim is remarkable for a mica mine, which is said to produce the largest and clearest sheets of the substance in the world, some of them being quite pure to the extent of two feet and a half square.—*Narrative of a Journey round the World, by Sir George Simpson,* vol. ii., p. 334.

4. *Lake Baikal.*—Lake Baikal is about seven or eight hundred versts in length, and about seventy or eighty broad at its widest part. The waters are as clear as crystal, everywhere deep, and in many places unfathomable. Besides the numberless cascades that rush down its walls of mountains, it receives many rivers, more especially the Angara at its northern extremity, and the Selenga on its eastern side, towards the south; and its single outlet, in spite of the superior claims of the Selenga, on the double ground of position and magnitude, professes in its name to be a continuation of the remote and comparatively inconsiderable Angara. The two Angaras are sometimes distinguished from each other as Upper and Lower. The quantity of water which issues from the lake is believed to be vastly less than that which flows into it, the difference being, in all probability, too great to be explained by evaporation alone. In this view of the thing, a large portion, as a matter of course, must be absorbed, an operation which the volcanic origin of the huge hollow may be supposed likely to facilitate. In fact, the lake presents certain features which have induced individuals to infer that it has a subterranean communication with the ocean. It is the only body of fresh water in the world that possesses phocœ or seals; and when agitated, in the way already mentioned, by invisible curves, it throws up to its surface quantities of small fish, which are never seen at any other time. In illustration of the mysterious agencies of nature, which produces the same ends by contrary means, I subjoin two passages from Baron Wrangell's interesting work.

"These flat valleys are occasionally filled with water, by the overflowing of the rivers in spring, when they form lakes of various sizes, all very full of fish. The intense frosts of winter cause large clefts in the ground, by which the water drains off, sometimes in the course of a single year, sometimes in several.

"A curious phenomenon occurs in the lakes in the vicinity of the village of Alaseya. In the middle of winter, the water sometimes suddenly disappears without any side channels being visible. In such cases a loud noise is heard at the time the water disappears, and, when the bottom of the lake is laid bare, large clefts are visible, occasioned by the severity of the frost."—*Narrative of a Journey round the World, by Sir George Simpson*, vol. ii., p. 381.

5. *Chiolite, a new mineral from Miask;* By Fr. v. Wörth.—Chiolite is a massive mineral resembling cryolite. It occurs granular, with a shining lustre, and presenting a greyish, yellowish, or snow-white colour. The streak is snow-white. The hardness is between calc-spar and fluor-spar. Specific gravity of the masses, 2·6209; of the powder, 2·770. The structure of the massive pieces is imperfectly foliated, and the surface of foliation exhibits a lustre between greasy and vitreous. The specimen exhibited by M. Wörth indicated that it was associated with lithic mica, massive fluor-spar, and quartz. It melts easily in the flame of a candle, but at first decrepitates. Its transparency increases in water.

According to the chemical analysis of A. Chodner, it consists of aluminum 16·48, sodium 25·72, potassium 0·58, magnesium 0·76, yttrium 1·04, fluorine 56·00=100·38, giving the formula 2 Na Fl, and also in its higher specific gravity. The name chiolite is from the Greek word *to snow.*—*Verhandl. Min. Ges. zu St Petersburg*, 1845–46.

6. *Arsenic in Mineral Waters.*—Arsenic has been found by M. Valchner in various mineral waters at Viesbade in Germany, and this has been confirmed by M. Figuer. The last-mentioned chemist has ascertained that arsenic is in the state of arsenous acid, and that the proportion is nearly 0·045 grammes in 100 litres of the water. He detected no arsenic in the waters of Passy.—*L'Institut*, No. 670.

7. *Nitrification.*—M. Dumas states, that when a current of moist air containing ammonia is directed upon a solution of potash, the temperature being at 100° C., a quantity of nitrate of potash is formed through a change of the ammonia into nitric acid. He remarks that this experiment, which accords with the labours of M. Kuhlman on nitrification, was suggested to him by observations which he had recently made upon the conversion of sulphuretted hydrogen into sulphuric acid.—*L'Institut*, No. 674.

8. *Phosphate of Lime in Organic Beings.*—M. Dumas attributes the disaggregation of bones on exposure in the soil, and the removal of the phosphate of lime by water, to two causes, the one of feeble intensity, and acting rarely; the other of great force, and always in action. The first depends on the *ammoniacal* salt in waters, which salt enables them to dissolve phosphate of lime; this salt is everywhere present, but in so small a quantity as to have comparatively little influence. The second depends on carbonic acid, which appears to be the true solvent of phosphate of lime; for waters charged with carbonic acid, dissolve large quantities of it. Alkalies and ebullition separate the carbonic acid, and precipitate the salt. The action of this acid is so powerful, that shavings of ivory placed in a bottle of

Seltzer water, are softened in twenty-four hours, as if in ch orohy-dric acid; and the Seltzer water contains, afterwards, all the pho phate of lime contained in the ivory. This property, adds Du enables us to understand the introduction of phosphate of li plants. These facts explain the disaggregation of bones, a dissemination of the phosphate of lime in the soil through th bonic acid contained in rain waters; they shew how, in the economy, bones may be redissolved by the venous blood charge carbonic acid; they indicate the part which the fluoride of ca acts in the teeth, in protecting the osseous portion from the ca acid disengaged from the lungs, and dissolved also in the which at the same time is alkaline, to neutralize the action of th Dumas suggests the use of carbonated waters for persons af with calculi of phosphate of lime.—*L'Institut*, No. 674.

ZOOLOGY.

9. *Vampire Bats.*—At sunset swarms of bats flutter th field and forest in all directions, and greedily devour the insects in the twilight awaken to full activity. Some of these bats Ph *lostoma hastatum*, Geoff.) are remarkable for their expanse o wing which measures nearly two feet. Others are distinguished for ugliness and for their offensive smell. These latter fly into the Indian huts at night and greatly annoy the inhabitants, who annot get rid of them by fire or smoke, or any other means, until at the midnight hour they retire of their own accord. Not less troubleson are the leaf-nosed bats (Phyllostoma), which attack both man an beast. This bat rubs up the skin of his victim, from which he suc the blood. The domestic animals suffer greatly from the noctur attacks of these bats, and many are destroyed by the exhaustio consequent on the repeated blood-sucking. The blood drawn by bat itself does not exceed a few ounces; but if, when satisfied, drops down to the ground or flies away, the wound continues to ble for a long time, and in the morning the animal is often found in very weak condition, and covered with blood. One of my mules, which a leafed-nosed bat made a nightly attack, was only saved b having his back rubbed with an ointment made of spirits of campho soap, and petroleum. The blood-suckers have such an aversion t he smell of this ointment, that on its application they ceased to ap-oach the mule. These bats are very mischievous in the planta-ons of the forests, where beasts of burthen and horned cattle are posed to their attacks. Whether they venture to assail man has een a much disputed question. Several travellers declare that they do not; I may, however, mention a case which occurred within my own knowledge. A bat (*Ph. erythromos*, Tsch.) fastened on the nos of an Indian lying intoxicated in a plantation, and sucked so muc blood that it was unable to fly away. The slight wound was f

lowed by such severe inflammation and swelling, that the features of the Cholo were not recognisable.—*Travels in Peru, by Von J. J. Tschudi*, p. 418.

BOTANY.

10. *Quinua, or Chenopodium Quinoa, as a partial substitute for the Potato.*—Maize is the species of grain most extensively cultivated in the Sierra; it is of excellent quality, though smaller than that grown on the coast. Wheat, though it thrives well, is cultivated only in a very limited quantity, and the bread made from it is exceedingly bad. The other species of European grain, barley excepted, are unknown to the Serranos. To compensate for the want of them, they have the quinua (*Chenopodium Quinoa*, L.), which is at once a nutritious, wholesome, and pleasant article of food. The leaves of this plant before it attains full maturity, are eaten like spinach; but it is the seeds which are most generally used as food. They are prepared in a variety of ways, but most frequently boiled in milk or in broth, and sometimes cooked with cheese and Spanish pepper. The dried stems of the quinua are used as fuel. Experiments in the cultivation of this plant have been tried in some parts of Germany, and with considerable success. It would appear, however, that its flavour is not much liked; a circumstance rather surprising to the traveller who has tasted it in Peru, where it is regarded in the light of a delicacy. It were to be wished that the general cultivation of the quinua could be introduced throughout Europe; for during the prevalence of the potato disease this plant would be found of the greatest utility. It is a well known fact that potatoes and tea, two articles now in such universal use, were not liked on their first introduction into Europe. The quinua plant, which yields a wholesome article of food, would thrive perfectly in our hemisphere, and, though in its hitherto limited trial it has not found favour, there is no reason to conclude that it may not at a future time become an object of general consumption.—*Travels in Peru, by Dr J. J. Von Tschudi*, p. 367.

11. *Separation of Silica from Plants.*—Dr C. T. Jackson described a new method of separating silex from the stems of reeds, rushes, straw, and grass, which he had contrived while aiding the manufacturer of paper and hemp from reeds. He thought it might prove a convenient mode of demonstrating the presence of silex in plants to a class in the lecture room, and that it could be applied in chemical analysis of such plants as have a covering of silex. The process is as follows:—

The reeds are crushed or split, moistened with water, and placed in a leaden tube or cylindrical reservoir, and a smaller tube is connected with one end of it and carried down into a glass of water. Then a leaden bottle, being charged with pulverized fluor-spar and concentrated sulphuric acid, is connected with the opposite end of the reservoir, and the bottom of the bottle is carefully heated by a spirit-lamp, or by means of sand heated below the melting point of lead. The fluo-hydric acid gas passes over and through the reeds in the reservoir, and takes up all the silex in the state of fluo-hydrosilic acid gas, which passing into the water is decomposed, and hydrate of silica is deposited as a gelatinous mass, separable by means of a filter. The vegetable fibre, on being removed from the reservoir and washed, is destitute of silica.—*Proceedings of the Sixth Annual Meeting of the Association of American Geologists and Naturalists, April* 1845, p. 44.

12. *Collecting of Peruvian Bark.*—In the month of May, the Indians assemble to collect Peruvian bark, for which purpose they repair to the extensive Cinchona woods. One of the party climbs a high tree, to obtain, if possible, an uninterrupted view over the forest, and to spy out the *Manchas*, or spots where there are groups of Peruvian bark trees. The men who thus spy out the trees are called Cateadors or searchers. It requires great experience to single out the dark leaf-covered expanse, the Cinchona groups, merely by the particular tint of the foliage, which often differs but very little from that of the surrounding trees. As soon as the Cateador has marked out and correctly fixed upon the mancha, he descends to his companions, and leads them with wonderful precision through the almost impenetrable forest to the group. A hut is immediately built, which serves as a resting-place during the night, and is also used for drying and preserving the bark. The tree is felled as near the root as possible, divided into pieces, each from three to four feet long, and with a short curved knife a longitudinal incision is made in the bark. After a few days, if the pieces are found to be getting dry, the bark already incised is stripped off in long slips, which are placed in the hut, or in hot weather, laid before it to dry. In many parts, particularly in the central and southern districts of Peru, where the moisture is not very great, the bark is dried in the forest, and the slips are packed in large bundles. In other districts, on the contrary, the bark is rolled up green, and sent to the neighbouring villages, where it is dried. Towards the end of September, the *Cascarilleros** return to their houses.

In the more early periods of South American history, the bark was a principal article of Peruvian commerce. Since the commencement of the present century, its value has, however, considerably diminished, chiefly in consequence of adulterated and inferior kinds, which are supplied from other quarters ; perhaps, also, on account of the more frequent use of quinine ; for, in the production of the alcaloides less bark is employed than was formerly used in substance.

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* Bark-gatherers. The Peruvians call the bark cascarilla, and they point out the distinction of a great number of species and varieties.

During the war of Independence, the bark trade received its death-blow, and for the space of several years, scarcely more than a few hundredweights of bark were exported from Peru. The Montanas of Huanuco, which once furnished all the apothecaries of Europe with the "divine medicine," are beginning again to yield supplies. From the roots of the felled trees a vigorous after-growth has commenced. In the Montanas of Huamalies a kind of bark is found, the nature of which is not yet defined by botanists; and from the Montanas of Urubamba comes the highly esteemed *Cascarilla de Cuzco*, which contains an alcaloid, named *Cusconin**. Possibly the medicinal bark may again become a flourishing branch of trade for Peru, though it can never again recover the importance which was attached to it a century ago. During my residence in Peru, a plan was in agitation for establishing a quinine manufactory at Huanuco. The plan, if well carried out, would certainly be attended with success. There is in Bolivia an establishment of this kind conducted by a Frenchman; but the quinine produced is very impure. The inhabitants of the Peruvian forests drink an infusion of the green bark, as a remedy against intermitting fever. I have found it in many cases much more efficacious than the dried kind, for less than half the usual dose produces, in a short time, convalescence, and the patient is secure against returning febrile attacks.—*Travels in Peru, by Dr J. J. Von Tschudi*, p. 398.

ARTS.

13. *On Shell Cameos.—Society of Arts, April* 21, 1847. Dr Roget, V.P. in the chair.—Mr Gray "On the Manufacture of Shell Cameos." The author commenced by stating that the ancients formed cameos by engraving figures in low relief on different kinds of siliceous stones; and generally selected for that purpose those which had layers of different colours, so that the figures were of divers colours. Such cameos are now made in Southern Europe and in France, where this art has lately been attempted to be revived; but the hardness of the materials requires so much labour that they are too expensive to come into general use. Numerous attempts have been made to substitute various materials, such as porcelain and glass, for the ancient cameos, but their great inferiority has caused them to be neglected. The best and now most used substitutes are shells; several kinds of which afford the necessary difference of colour, and are, at the same time, soft enough to be worked with ease, and hard enough to resist wear. The shells used are those of the Flesh-eating univalve, which are peculiar as being formed of three layers which are often of different colours, as they afford him the means of reliev-

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* From Cuzco, the ancient residence of the Incas. It was discovered by the French chemists Corriol and Pelletier, in the Cascarilla which is shipped in Arica; hence this alcaloid is also called *Aricin*.

ing his work; but the kinds now employed, and which experience has taught him are best for his purpose, are the Bull's Mouth, the Black Helmet, the Horned Helmet, and the Queen Conch. The two first are the best shells. After detailing the peculiarities of these shells, the writer proceeded to give an account of the progress of the art, which was confined to Rome for upwards of forty years, and to Italy until the last twenty years, at which period an Italian commenced the making of them in Paris; and now about 300 persons are employed in this branch of trade in that city. The number of shells used annually, thirty years ago, was about 300, the whole of which were sent from England; the value of each shell in Rome being thirty shillings. To shew the increase of this trade, the number of shells used in France last year was nearly as follows:

Bull's Mouth	80,000,	average price	1s. 8d.,	value	L.6400
Black Helmet	8,000,	...	5s. 0d.,	...	1800
Horned Helmet	500,	...	2s. 6d.,	...	60
Queen Conch	12,000,	...	1s. 2½d.,	...	700
	100,500 shells.			Sterling	L.8960

The average value of the large cameos made in Paris is about 6 francs each, giving a sterling value of L.32,000; and the value of the small cameos is about L.8000, giving a total value of the cameos produced in Paris for the last year of L.40,000; while in England not more than six persons are employed in this trade.—*Athenæum*, No. 1018, p. 470.

14. *Smelting by Electricity.*—The lately patented process of smelting copper by means of electricity, says a London journal, is likely to effect a change that will be quite prodigious. It produces, in less than two days, what the old process required three weeks to effect. And the saving of fuel is so vast, that in Swansea alone, the smelters estimate their annual saving in coals at no less than five hundred thousand pounds. Hence, it is clear, that the price of copper must be so enormously reduced, as to bring it into use for a variety of purposes from which its cost at present excludes it. The facility and cheapness of the process, too, will enable the ore to be largely smelted on the spot. The Cornish mine proprietors are anxiously expecting the moment when they can bring the ore which lay in the mine yesterday into a state to be sent to market to-morrow, and this at the very mouth of the mine. In Australia, also, the operation of this discovery will be of the utmost importance. Ten thousand tons of copper-ore were sent from Australia to England last year, to be smelted at Swansea; and the result was only 1600 tons of copper. But Australia in future will smelt her own copper, by a 36 hours' process: saving all this useless freight of the 8400 tons of refuse, and saving also the cost of the old and expensive process. In a very few years, Australia will send to market

more copper than is now produced by all the rest of the world. But if our future penny-pieces are to bear any proportion to the reduced cost of the value of the metal, they must be made of the size of dinner-plates! !—*Athenæum*, No. 1018, p. 476.

15. A communication from Professor Schottlauer, of Munich, acquaints us with particulars of a new invention for painting upon walls, discovered by himself, conjointly with Herr Fuchs, Counsellor of the Mines, to be called Stereochromy. Its peculiarities are stated as follows:—Far greater ease in its manipulation than fresco. The ground is not laid in patches, but by one single operation. The colours, prepared in distilled rain-water, take such firm hold as not to be disturbed or altered by any subsequent washings or shades, while the process of painting may be carried on with any amount of intervals, thus rendering a far richer finish possible than with fresco. After the picture is finished, it is saturated with a fluid, which unites the ground and the colour into a mass of the consistency of stone; desiccations being thereby rendered impossible. The colours are of greater strength and brightness than with fresco,—though without the slightest glare or reflection as of oil. It resists all atmospheric influences,—humidity, evaporation, &c. A test no less extreme than the burning of alcohol, has been applied to it, without the slightest change or deterioration.—*Athenæum*, No. 980, p. 820.

NEW PUBLICATIONS RECEIVED.

1. The Ancient World; or Picturesque Sketches of Creation. By Professor Ansted, of King's College, London. 1 vol. 8vo, pp. 408. With numerous Woodcuts. John Van Voorst. London, 1847. *A pleasant and agreeable volume, well deserving the attention of cultivators of Geology.*

2. Outlines of Structural and Physiological Botany. By Arthur Henfrey, F.L.S., &c., Lecturer on Botany at St George's and the Middlesex Hospitals, &c. With numerous Illustrations. Pp. 245. John Van Voorst. London, 1847. *Of this excellent elementary work we made mention on a former occasion. As we expected, it is becoming a class-book in our Universities.*

3. Manual of British Botany, containing the Flowering Plants and Ferns, arranged according to the Natural Orders. By C. C. Babington, M.A., F.L.S., F.G.S., &c. 2d edition, with many Additions and Corrections. Pp. 428. John Van Voorst. London, 1847. *An improved edition of Mr Babington's well-known and esteemed volume.*

4. The Birds of Jamaica. By Philip Henry Gosse, assisted by Richard Hill, Esq., of Spanish Town. 1 vol. 8vo, pp. 447. John Van Voorst. London, 1847. *We hope the excellent example set by the publication of this interesting work will be followed by similar ones from other West India Islands.*

5. A History of the British Zoophytes. By George Johnston, M.D., LL.D., Fellow of the Royal College of Surgeons of Edinburgh. Second edition. 2 vols., 8vo, with numerous engravings. John Van Voorst.

London, 1847. *It affords us great pleasure to announce the publication of this improved and extended edition of Dr Johnston's standard work on British Zoophytes.*

6. The London Geological Journal, or Record of Discoveries in British and Foreign Palæontology. Two numbers, published by John Churchill, Princes Street, Soho, London. *We trust this promising Journal will be so supported by the numerous cultivators of Palæontology in this country and on the Continent, as to secure its continuance.*

7. Report on the Cultivation and Manufacture of Tea in Kemaoon and Gurhwall. By William Jameson, Esq., Superintendent, Botanical Gardens, North Western Provinces. *This important Report will be read with great pleasure by all those interested in the very promising appearance of the Tea Plantations in the North West Provinces of India, which, under the liberal and fostering care of the East India Company, and of their very energetic superintendent, Mr Jameson, will, we trust, ere long, enable India to supply Europe with excellent and* CHEAP *Teas.*

8. The Fourteenth Annual Report of the Royal Cornwall Polytechnic Society, 1846.

9. Geological Results of the Earth's Contraction in consequence of Cooling. By James D. Dana, New Haven, United States.

10. On three several Hurricanes of the Atlantic, and their relations to the Northers of Mexico and Central America; with notices of other storms. By W. C. Redfield, New Haven, United States. *We prize highly this important work of our indefatigable and able correspondent.*

11. Observations on the probable cause of the failure of the Potato Crop, in the years 1845 and 1846. By David Milne, Esq., of Milnegraden, President of the East of Berwickshire Farmers' Club, &c. *One of the best and most scientific volumes on the subject of which it treats.*

12. Proceedings of the American Philosophical Society from January 1845 to June 1846.

13. Proceedings of the Academy of Natural Sciences of Philadelphia, May and June 1846.

14. Some Remarks on the present and future prospects of the Observatory of the Astronomical Institution of Edinburgh.

15. A Letter to Augustus de Morgan, Esq., of Trinity College, Cambridge, Professor of Mathematics in University College, London, on his claim to an independent re-discovery of a new principle in the theory of Syllogism. From Sir William Hamilton, Bart.

16. Erect Vision from an Inverted Image. By B. F. Joslin, M.D., of New York.

17. Observations on Life, as the cause of Vital Phenomena. London. *This particularly interesting Tract is intended for private friends, and may be considered as the precursor of a great work on Life. The author is a distinguished medical writer.*

18. Physical Atlas: A Series of Maps illustrating the Geographical Distribution of Natural Phenomena. By Dr Berghaus of Berlin, and Alexander Keith Johnston, F.R.G.S., &c. Part VI. *In our next number we shall give a Table of the Contents of this very beautiful work.*

N.B.—*The greater number of the Indian, German, French, and British Periodicals have been received up to a late date.*

List of Patents granted for Scotland, from 22d March to 22d June 1847.

1. To Charles Tennant Dunlop, of Glasgow, in the county of Lanark, manufacturer, "improvements in the manufacture of alkali and chlorine, and in the application of the products resulting therefrom."—23d March 1847.

2. To Thomas Hunt Barber, of King Street, Cheapside, in the city of London, merchant, being a communication from abroad, "improvements in machinery or apparatus for dredging or excavating."—23d March 1847.

3. To John Leslie, of Conduit Street, Hanover Square, in the county of Middlesex, tailor to Her Majesty, "improvements in the combustion of gas."—26th March 1847.

4. To Alexander Morton, of Morton Place, Kilmarnock, "improvements in printing warps."—26th March 1847.

5. To Francois Stanilas Meldon de Sussex, manufacturing chemist, "improvements in smelting copper and other ore."—31st March 1847.

6. To Alexander Bain, of Baker Street, in the county of Middlesex, electrical engineer, "improvements in clocks and time-keepers, and in apparatus connected therewith."—31st March 1847.

7. To William Farthing, of the town and borough of Kingston-upon-Hull, merchant, "certain improvements in the manufacture of glass."—1st April 1847.

8. To William Henry Hatcher, of No. 345 Strand, in the county of Middlesex, civil-engineer, "improvements in electrical telegraphs, and in apparatus connected therewith, and also in electric clocks and time-keepers."—6th April 1847.

9. To George Fergusson Wilson, of Bellmont, Vauxhall, in the county of Surrey, gentleman, "improvements in the production of light, and in the manufacture or preparation of materials applicable thereto."—6th April 1847.

10. To John Lowe, of Manchester, in the county of Lancaster, civil-engineer, and James Simpson of the same place, joiner, "certain improvements applicable to carriages to be used upon railways, part of which improvements may also be used upon other roads."—7th April 1847.

11. To Bartholomew Beniowski, of Bow Street, Covent Garden, in the county of Middlesex, major in the late Polish army, "certain improvements in the apparatus for, and process of printing."—12th April 1847.

12. To George Frederick Muntz, of Birmingham, Esquire, M.P., "an improved manufacture of metal-plates for sheathing the bottoms of ships and other vessels, and also in bolts and other the like ships' fastenings."—13th April 1847.

13. To Patrick Moir Crane, of Yuiscedwyn iron-works, near Swansea, "improvements in the manufacture of iron.—13th April 1847.

14. To William Pidding, of Alfred Place, Bedford Square, in the county of Middlesex, gentleman, "certain improvements in carriages."—13th April 1847.

15. To Charles Heard Wild, of Mortimer Street, Cavendish Square, in the country of Middlesex, civil-engineer, "improvements in constructing parts of railways."—16th April 1847.

16. To Onesiphorè Pecqueur, engineer, of Paris, in the kingdom of France, "improvements in forming leather into tubes, swithes, cases, sheaths, hats, and other articles."—16th April 1847.

17. To John Coats, of Seedly, in the county of Lancaster, calico printer, "certain improvements in machinery or apparatus for cleaning the surface of woven fabrics, or freeing the same from fibrous or other loose matters previous to printing thereon."—29th April 1847.

18. To Joseph Woods, of Bucklensbury, in the city of London, engineer, being a communication from abroad, "certain improvements in springs for supporting heavy bodies, and resisting sudden and continuous pressure."—30th April 1847.

19. To Louis Sylvain Gonin, manufacturer, of Paris, in the kingdom of France, "improvements in printing stuffs, paper, and other matters."—4th May 1847.

20. To Philip Burnard Ayres, of No. 12 Howland Street, Fitzroy Square, in the county of Middlesex, doctor of medicine, "certain plans and improvements in preparing putrescent organic matters, such as night soil, the matter in suspension in the water of sewers and other similar matters, for the purpose of manure or for other purposes, and for apparatus for the same."—4th May 1847.

21. To George Copeland, of 37 Frederick Street, Edinburgh, "an instrument or apparatus for measuring the human body, for the purpose of fitting garments with ease and accuracy, without reference to the proportions of the breast and shoulder measures, and which invention he names the *Corporimensor*."—5th May 1847.

22. To Thomas Waterhouse, of Edgeley, in the borough of Stockport, in the county of Chester, cotton manufacturer, "certain mechanical improvements applicable to railway engines and tenders, and to railway carriages of various kinds."—8th May 1847.

23. To Maximilian Francois Joseph Delfosse, of Regent Street, in the county of Middlesex, Esquire, "improvements in preventing and removing incrustation in steam-boilers."—13th May 1847.

24. To Conrad Haverkam Greenhow, of North Shields, gentleman, "improvements in the construction of ships or vessels, and in propelling ships and vessels."—14th May 1847.

25. To Samuel Hardacre, of Manchester, in the county of Lancaster, machinist, "certain improvements in machinery or apparatus for opening and for carding cotton and other fibrous substances, and for grinding the cards of carding engines."—17th May 1847.

26. To George Benjamin Thorneycroft, of Wolverhampton, in the county of Stafford, iron-master, "improvements in the manufacture of rails for railways."—18th May 1847.

27. To Gardner Stow, of King Street, Cheapside, in the city of London, gentleman, being a communication from abroad, "improvements in the construction of steam-vessels, and in apparatus for propelling ships and other vessels."—18th May 1847.

28. To Reginald James Blewitt, of Llantarnam Abbey, Newport, in the county of Monmouth, Esquire, "improvements in the manufacture of malleable iron."—24th May 1847.

29. To Solomon Leatham, of Leeds, in the county of York, overlooker, "improvements in roving and spinning flax and other fibres."—24th May 1847.

30. To Christian Schiele, of Frankfort-on-the-Maine, but now of Manchester, in the county of Lancaster, mechanician, "certain improvements in machinery, or apparatus for condensing steam, which said improvements are also applicable to other similar purposes."—28th May 1847.

31. To Jean Marie Fourmentin, of New Bridge Street, Blackfriars, gentleman, "improvements in the manufacture of carbonate of lead."—28th May 1847.

32. To Thomas Bartlett Simpson, of Threadneedle Street, in the city of London, gentleman, "certain improvements in propelling, and in machinery employed therein."—2d June 1847.

33. To Edmund Morewood, of Thornbridge, in the county of Derby, merchant, and George Rogers, of Stearndale, in the same county, gentleman, "improvements in the manufacture of iron into sheets, plates, or other forms, in coating iron, and in preparing iron for coating and other purposes."—7th June 1847.

34. To George Augustus Huddart, of Brynkir, in the county of Carnarvon, Esquire, "certain improved apparatus for the cultivation of land."—7th June 1847.

35. John Hill, of Hulme, near Manchester, in the county of Lancaster, machine-maker, "improvements in looms for weaving certain kinds of cloth."—9th June 1847.

36. To Francis Bowes Stevens, of Haboken, in the county of Hudson, in the State of New Jersey, in the United States of America, engineer (being a communication), "improvements in applying means and apparatus to ships and vessels to improve their speed."—9th June 1847.

37. To Elijah Galloway, of Buckingham Street, Strand, in the county of Middlesex, civil-engineer, "improvements in rotatory engines, and in locomotive carriages, railways, and wheels for carriages."—11th June 1847.

38. John Lane, of Oriel Street, Liverpool, brewer, "improvements in railway carriages and engines."—11th June 1847.

39. To Alfred Vincent Newton, of the office for Patents, No. 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, being a communication from abroad, "improved apparatus to be applied to steam-boilers."—11th June 1847.

40. To John Healey, of Bolton, in the county of Lancaster, machine-maker, "a new and improved woven fabric, and also certain improvements in machinery for producing the same."—15th June 1847.

41. To Charles Larrad, of Leicester, machinist, "improvements in machinery for cutting wood for the manufacture of bobbins and other articles."—15th June 1847.

42. Thomas Russell Crampton, of the Adelphi, in the county of Middlesex, engineer, "improvements in locomotive engines,"—15th June 1847.

43. To Alfred Brett, of Holborn Bars, gentleman, and George Little, of High Holborn, electrical engineer, "improvements in electric telegraphs, and in arrangements and apparatus to be used therein and therewith, part of which improvements are also applicable to time-keepers and other useful purposes."—15th June 1847.

44. To Pierre Frederic Gougy, of Leicester Square, in the county of Middlesex, gentleman, "improvements in apparatus and machinery for raising, *lifting, and otherwise moving* heavy bodies,"—16th June 1847.

45. To Samuel Kenrick, of Handsworth, in the county of Stafford, ironfounder, "certain improvements in preparing or forming moulds for casting metal."—18th June 1847.

46. George Taylor, of Holbeck, near Leeds, in the county of York, mechanic, "improvements in the construction of engines and carriages to be used on railways."—18th June 1847.

47. To Frederic Theodore Philippi, of Bellfield Hall, in the county of Lancaster, calico-printer, "certain improvements in machinery or apparatus for stretching, drying, and finishing woven fabrics."—18th June 1847.

48. To Francis Preston, of Ardwick, near Manchester, spindle maker, "certain improvements in machinery or apparatus to be used in the preparation of cotton and other fibrous substances for spinning,"—21st June 1847.

LIST OF PRIZES FOR SESSION 1847-8.

The ROYAL SCOTTISH SOCIETY of ARTS proposes to award Prizes of different values (none to exceed Thirty Sovereigns), in Gold or Silver Medals, Silver Plate, or Money, for approved Communications, relative to Inventions, Discoveries, and Improvements in the *Mechanical* and *Chemical* Arts in General, and also to means by which the *Natural Productions* of the Country may be made available; and, in particular, to,—

I. Inventions, Discoveries, or Improvements in the Useful Arts, including the *Mechanical* and *Chemical*; and in the Mechanical Branch of the *Fine Arts;* such as the following, viz.:—

1. *Mechanical Arts.*

1. Methods of Economising Fuel, Gas, &c.,—of Preparing Superior Fuel from Peat,—of Preventing Smoke and Noxious Vapours from Manufactories,—of Warming and Ventilating Public Edifices, Private Dwellings, &c.—of Constructing Economical and Salubrious Dwellings for the Working Classes, especially in Towns,—of Filtering Water in large quantities,—of rendering large supplies of Water available for the purpose of extinguishing Fires; and the best application of Manual or other Power to the working of Fire-Engines,—of Constructing Buildings on the most correct Acoustic principles.

2. Inventions or Improvements in the Manufacture of Iron, and other Metals, simple or alloyed,—in the Manufacture of Writing and Printing Paper,—in Tuyeres for Blast Furnaces,—in the Making and Tempering of Steel,—in Gilding Brass equal in Colour to the French,—in Artificial Pavement,—in Balance or Pendu-

lum Time-Keepers, moved by Weights or Springs; or in Electro-Magnetic Time-Keepers,—in Screw-cutting,—in Printing-Presses,—in Stereotyping, and in cleaning the plaster from the Types,—in Furnaces and other Apparatus used in Stereotyping, —in Type-Founding,—in the Composition of Printers' Rollers, —in Ship-Building, with regard to Ventilation, both for the Crew and the Timbers,—in Currying and Tawing of Leather,— in Preparing Black Polished Leather equal to the French,—in Stationary and Locomotive Engines,—in Railway Wheels and Axles,—in Railway Telegraphs and Signals,—in Smith-Work and Carpentry,—in Tools, Implements, and Apparatus for the various trades,—in Electric, Voltaic, and Magnetic Apparatus.

2. *Chemical Arts.*

IMPROVEMENTS in Fine Glass for Optical Purposes, free from Veins, and of a Dense and Transparent quality, equal or superior to the best Continental Glass,—also in rendering Glass hard and difficult of fusion for Chemical Purposes,—in the Annealing of Glass, —in the Manufacture of Writing Inks, both Common and Copying, so as to flow freely from Metallic Pens,—in the Dissolving of Caoutchouc, and applying it to useful purposes.

3. *Relative to the Fine Arts.*

IMPROVEMENTS in Patterns of Porcelain, Common Clay, or Metal, of Domestic Articles of simple and beautiful Forms, without much Ornament, and of one Colour,—in the Preparation of Lime and Plaster for Fresco Painting, and in appropriate tools for laying the Plaster with precision,—in Engraving on Stone,— in Daguerreotype, Talbotype, or other photographic processes,— in applying such processes to stone, for Lithographic Printing,— in Electrotype processes,—in the production of White or Neutral Artificial Light by means adapted to ordinary use,—in Die-sinking,—in Wood-cutting, and other methods of illustrating Books to be printed with the Letter-Press,—in Printing from Wood-cuts, &c.,—in Ornamental Metallic Casting.

II. EXPERIMENTS applicable to the Useful Arts.

III. NOTICES of Processes in the Useful Arts practised in this Country, but not generally known.

IV. INVENTIONS, Processes, or Practices from Foreign Countries, not generally known or adopted in this country,—such as the Manufacture of Glass Pipes for Conveying Water, Gas, &c.

V. PRACTICAL DETAILS of Public or other Undertakings of National importance, not previously published.

VI. DISCOVERY of Substitutes for Hemp and Flax, &c.

The SOCIETY also proposes to award the KEITH PRIZE, value THIRTY Sovereigns,

For some important "Invention, Improvement, or Discovery, in the Useful Arts, which shall be primarily submitted to the Society," betwixt and 1st April 1848.

GENERAL OBSERVATIONS.

The Communications and the Descriptions of the various inventions, &c., to be *full* and *distinct*, and to be written on *Foolscap* paper, leaving margins at least one inch broad, on *both the outer and inner sides of the writing*, so as to allow of their being bound up in volumes; and, when necessary, to be accompanied by *Specimens, Drawings, or Models*. All drawings to be on *Imperial* Drawing Paper, unless a larger sheet be requisite. The Drawings, and the Letters or Figures of Reference, to be in *bold* lines, or *strongly coloured*, so as to be easily seen from some distance when hung up in the Hall of Meeting.

The Society to be at liberty to publish in their Transactions copies or abstracts of all Papers submitted to them. All Models, Drawings, &c., for which Prizes shall be given, to be held to be the property of the Society; the Value of the Model being taken into account in fixing the amount of the Prize.

Communications, Models, &c., are to be addressed to JAMES TOD, Esq., the SECRETARY, 21 Dublin Street, Edinburgh, Postage or Carriage paid; and they are expected to be lodged *on or before* 1st *October* 1847, in order to ensure their being read and reported on during the Session, the ordinary Meetings of which end in April 1848; but, those *which cannot be lodged earlier*, will be received up to 1st March 1848.

By order of the Society,

JAMES TOD, *Secretary*.

EDINBURGH, 12*th April* 1847.

*** Copies of this List of Prizes may be had from the Secretary.

THE

EDINBURGH NEW

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On the Nucleus of the Animal and Vegetable "Cell." By MARTIN BARRY, M.D., F.R.S., London and Edinburgh. Communicated by the Author.* With Two Plates.

"It will be remarkable if the mammiferous ovum, which, because of its minuteness and the supposed difficulty of obtaining it, had been generally considered beyond the reach of satisfactory observation, should now become the means of studying, not merely other ova, but certain processes by which nourishment is communicated, and the growth of the body effected at all future periods of life. Such, however, I think will really be the case."

This was long since written by myself,† under a conviction that the nuclei of all cells were the seat of changes of the same kind as those which I had shewn to attend the fecundation of the mammiferous ovum, in the nucleus of its germinal vesicle or cell—the germinal spot. Whether in the opinion then expressed I was mistaken, will be seen from the following pages; intended to contain not only an outline of the elaborate process producing the changes in question, but also a few examples of the extent to which I have the satisfaction to find my observations have been confirmed, and my views formally applied.

I think it peculiarly proper to offer this communication to the Wernerian Society; having been first induced to enter upon microscopic research by the perusal of a work on Embryology put into my hands by the revered President of this Society, Professor Jameson.

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* The substance of a paper read before the Wernerian Society of Edinburgh, on Saturday, the 3d April 1847, with some additions.

† Phil. Trans. 1841, p. 201.

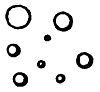
The nucleus of the vegetable cell had long been noticed before it received any particular attention, which was first given to it by our distinguished fellow-countryman, Robert Brown; who states this body to have been so neglected, that there are instances of botanists having actually figured the nucleus in the cell, without thinking it worth noticing in the explanation of their plates. The structure in question did not obtain the attention of physiologists in general until the year 1838, when Schleiden published his observations on the nucleus of the vegetable cell; which appear to have given rise to,—for they were immediately followed by,—the extensive and invaluable researches on cells in animal tissues, by my friend Professor Schwann. And never, perhaps, did the microscope yield a richer harvest of observations than on that occasion in his able hands; for to Schwann we are indebted for the great discovery, that the elementary parts of tissues have a like origin in cells, however different the functions of those tissues. It is with regret that I place his name in the list of those from whose observations on *cellular development* my own widely differ,—the differences relating to points of cardinal importance.

Schleiden shewed the nucleus to be the part that gives origin to the membrane of the cell, which membrane when in an incipient state he compared to a watch-glass on a watch. But Schleiden supposed that when the nucleus has given origin to the membrane of the cell it has performed its office; and that, not being further required, the nucleus then either remains inert in the cell-wall, or is "cast off as a useless member," and "absorbed." I am not aware that that opinion of Schleiden was questioned in any quarter, until my own observations had shewn it to be the very opposite of the truth. So far from the nucleus of the cel either remaining inert in the cell-wall, or being absorbed a useless, it is a most active agent, and endowed with properties of the first importance; while the membrane it forms i a structure subordinate thereto.

From its property of originating the membrane of the cell, Schleiden gave to the nucleus the name of Cytoblast or cell-germ. In the cytoblast he saw what seemed a smaller body, the nucleolus; and he, as well as Schwann, Müller, Valentin, and Henle, supposed the smaller body to exist first,—the substance of the cytoblast, or larger body, they believed to be deposited *around* the smaller one,—and the substance of the membrane of the cell to be deposited *around* the cytoblast.*

According to my observations, it is not so: I find the process to be of a widely different character; so different, indeed, that it may be termed the very opposite of that supposed by the German physiologists just mentioned. It is true that a minute pellucid, and, as will presently be seen, a very important globule (fig. 1), exists before the cytoblast. But it is not true that a finely granular substance is "deposited around" that pellucid globule, so as to make the nucleolus seen in the cytoblast identical with the previously existing pellucid globule. That previously existing pellucid globule *absorbs and assimilates* new matter, and, at the same time, enlarging and becoming finely granular, assumes the form in fig. 4, the cytoblast of Schleiden,—having prepared a nucleolus for itself. Nor is it true that the membrane of the cell is *formed around* the cytoblast. *The outer part of the cytoblast* rises in the form of membrane, so as to produce fig. 5, a cell; another portion of the cytoblast enters into the formation of the contents of the cell; and what is left of the cytoblast in the cell-wall, becomes the nucleus of the cell.

Fig. 1.



This nucleus, said to remain inert in the cell-wall, or to be "cast off as a useless member" and "absorbed," is the seat of a most elaborate process, which I will now endeavour to describe: first remarking, that the woodcuts are intended for the purpose of assisting in the description of that process, rather than as minutely representing nature. Drawings in which the latter has been attempted, taken from hundreds in my memoirs, are given in the plates; and these will be rendered more intelligible by the woodcuts. (In many of the drawings, for instance in those of fig. 39, Plate I., the structure is so minute that it should be viewed with a pocket lens.)

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* The following is the substance of a statement made by Valentin, as briefly expressing the views on this subject of Schleiden, Schwann, Müller, Henle, and himself; and as being descriptive of the first formation of the elements of tissues:—In a fluid, says he, there are precipitated granules, which are nucleoli; *around* the nucleolus there is deposited a finely granular substance, by which there is formed the nucleus (cytoblast); and *around* the nucleus there is formed the membrane of the cell. The principle of formation of the nucleus *around* the nucleolus, is essentially the same as that of the cell *around* the nucleus. Valentin concludes that this process may be described by the expression, heterogeneous *circum-position.—Repertorium*, 1839, ii., p. 284.

The "cytoblast" of Schleiden has usually a discoid or amygdaloidal form, and in substance appears finely granular* (fig. 4); and his "nucleolus," the pellucid point in fig. 4, I find to represent the situation of a brilliantly pellucid substance, which, from its appearance, I have called *hyaline.*†

Fig. 2. Two portions of hyaline enlarged, and surrounded by globules of extreme minuteness, which seem to have their origin in the bodies they surround. One of these bodies presents a faint circular marking at the side, *not yet formed in the other*. This circular marking is the incipient nucleolus. Compare with α and β, the two states of nuclei, in Fig. 33, Plate I.; a nucleolus being there present in β, and not yet formed in α.

Fig. 2.

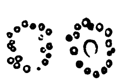


Fig. 3. That which was a large portion of hyaline in fig. 2, is now an incipient cytoblast, and already membranous at the surface; the minute globules, now no longer seen, having apparently entered into the formation of its membrane. Compare with many figures in Plate I.

Fig. 3.

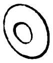
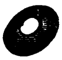


Fig. 4. The cytoblast of Schleiden; differing from the body fig. 3, in being finely granular, except in the region of the circular marking or nucleolus, which first made its appearance in one of the portions of hyaline fig. 2. Compare with figs. 35, 36, &c., Plate I.

Fig. 4.



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* In animals this finely granular substance is, or corresponds to, the red colouring matter of the corpuscles of the blood. See figs. 35, 36, &c., in Plate I

† A term suggested to me by Professor Owen.

Fig. 5. The cytoblast has become a cell; its contents finely granular,—the remains as they are of the finely granular substance of the cytoblast in the preceding figure.* *That which was the nucleolus in figs.* 2, 3, 4, *is now the nucleus.* This nucleus, a mass of hyaline, is surrounded by minute globules, as in fig. 2; and, like one of the bodies in that figure, *presents, in its turn, an ill-defined nucleolus.* Compare with two of the cells *β*, in fig. 34 of Plate I.

Fig. 5.

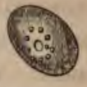

Fig. 6. The minute globules are no longer seen. They appear to have entered into the formation of the membrane by which the hyaline nucleus is now surrounded. This hyaline nucleus is in the same state as the entire body fig. 3,—*that of a young cytoblast.* It represents the "hollow nucleus" and "inner cell" of authors. *The nucleolus is now well-defined.* Compare with *γ* of fig. 34, in Plate I; and *read the description of that figure.*

Fig 6.

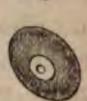

Fig 7. The hyaline nucleus of the cell has become finely granular, *and is now as much a cytoblast as the body, fig.* 4. Compare with the nucleus of several cells in fig. 39 of Plate I., and with the concentric cytoblasts in fig. 41 of the same Plate.

Fig. 7.

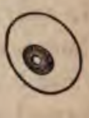

Fig. 8. The nucleus or cytoblast of fig. 7 has become a cell, and *that which was the nucleolus in fig.* 7, *is now the nucleus of this cell.* Farther, in the centre of the nucleus which had been a nucleolus, *another nucleolus is coming into view.* The hyaline, much increased in quantity, is giving off globules into the surrounding substance, and is hence nodular or star-like in its form. Compare with two cells on the right hand, and with other bodies in fig. 39 of Plate I.

Fig. 8.

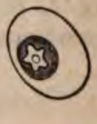

Fig. 9. The segments of the nodular or star-like hyaline

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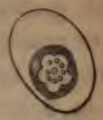
* The first contents of all cells have a like mode of origin; and it is not easy to say where the cytoblast terminates, and the cell begins; so that often one is obliged to use almost indiscriminately the terms "cytoblast" and "cell."

Fig. 9.



have assumed independent forms; having appropriated the finely granular substance into which they were cast, and thus become young cytoblasts. What was left of the central hyaline has increased in quantity, and, as in figs. 2 and 5, has become surrounded with minute globules. Compare with blood-corpuscles above the middle part of fig. 39, in Plate I.

Fig. 10. The young cytoblasts are larger. The minute globules are no longer seen, having probably entered into the formation of the membrane by which the central hyaline is now surrounded. This central hyaline is in the same state as the nucleus of the cell in fig. 6. It is a young cytoblast. Compare with several cells in fig. 39, of Plate I.

Fig. 10.

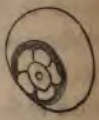
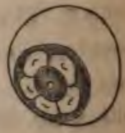


Fig. 11. What were young cytoblasts around the central hyaline, are now young cells; which surround what is now a true cytoblast; for the central hyaline, pellucid in fig. 10, is now finely granular. Compare with β in fig. 37, and with several cells in fig. 39 of Plate I.

Fig. 11.



Figs. 12, 13, 14, 15. To describe the central portion of the nucleus in these figures separately, would be to repeat the description just given of the entire nucleus in figs. 8, 9, 10, 11; for the appearances are the same, and referable to a continuation of the same process.

Fig. 12.

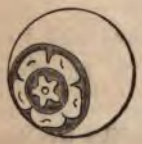


Fig. 13.

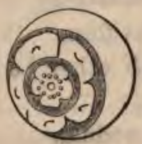


Fig. 14.

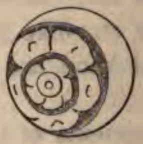
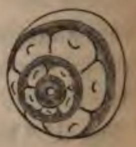


Fig. 15.



The only remarks to make concerning figs. 12 to 15, are ese, viz., that the layer of young cells already formed in g. 11, becomes pushed farther and farther into the interior f the cell by the succeeding brood; and that as each nucleus is transformed into a layer or brood of cells, its place is taken by the enlarging nucleolus; which again is succeeded by a fresh nucleolus, arising in the centre from which the former had enlarged. Compare with a cell in the woodcut fig. 26, and with a corresponding cell in fig. 39 of Plate I.

And this process, it will be seen, begins before the complete formation of even Schleiden's cytoblast. See figs. 2, 3.

The membrane of the nucleus or cytoblast, becoming the inner cell in fig. 8, and represented in all the succeeding figures,—though distinct enough, capacious, and for some important purpose *filled with pellucid fluid when in a central situation*, (Plate I., fig. 45 β),—is rarely seen with distinctness when subsequently pushed out from the centre by a fresh brood, and made to separate two layers of cells: and often it entirely disappears, as shewn by several bodies in fig. 39, Plate I.

I have just spoken of the cell-membrane, as formed by minute globules. An immeasurably minute cell-formation, however, seems even here to intervene. Fig. 38 in the Plate presents part of the membrane of a very important cell produced by the coalescence of minuter cells, the formation of which it is not difficult to follow.* And you afterwards find in its substance the nuclei, or germinal centres, of the cells which formed it (see the figure). Yet so very small is that cell at first, that two hundred millions of them could be contained in a cubic inch. To that cell (my *ovisac*†) I shall presently return. I wish now to add, that there is thus a considerable difference between my views and those of the German physiologists just mentioned, as to the mode of origin not only of the cytoblast, but of the membrane of the cell. The substance of the larger body is not deposited *around* the the smaller, but the smaller is transformed into the larger. The nucleolus *becomes* a cytoblast, and the cytoblast *becomes* a nucleated cell.

From the foregoing remarks and figures it will also be seen, and it is important to observe:—1. That in the early stages of its formation the cytoblast of Schleiden has no nucleolus, but acquires a nucleolus before its formation is complete. 2. That when the cytoblast has formed a cell, the nucleus of this cell is what had been the nucleolus of the cytoblast; and, 3. That now another nucleolus comes into view in the situation which had been occupied by its predecessor. 4. That this process continuing the same, the nucleus of one stage is the nucleolus of an earlier stage; there being a continual succession of nucleoli in the centre. 5. That it is a process which begins before the complete formation of even Schleiden's cytoblast; in fact begins with the existence of the minute pellucid globule, which thus becomes transformed into that cytoblast. 6. That this process is the very opposite of that described by Valentin in his statement of the united views of Schleiden, Schwann, Müller, Henle, and himself, as forming the elements of tissues.

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* See in Phil. Trans., 1841, Plate xxv., figs. 170, 171, 172, 173.

† Phil. Trans., 1838.

Figs. 1 to 15 represent the process in no more than the primary or main centre of each cytoblast and cell; they would have been complicated by any attempt to exhibit it in the secondary centres; where, however, abundant evidence is afforded that the process is the same, for the secondary cells fill with other broods in like concentric order, and leave no doubt whatever that every segment of the nodular or starlike hyaline in fig. 8, after having become a globule, passes through in succession the conditions represented in figs. 2, 3, and 4, before it becomes a cell. (And hence it seems to be, that the process is obscured by immeasurably minute and highly refracting globules).

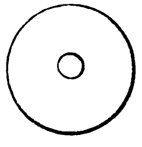
Wishing to confine the foregoing remarks as much as possible to the mode and order of formation of the cytoblast, the cell, the nucleus, and the nucleolus,—I have hitherto said nothing in this paper of an orifice often seen by me to connect the nucleolus with the exterior of the cytoblast and cell; and adding much to the refraction of light which takes place at that point. This orifice, which I first saw in the germinal spot in 1840, will be found represented in many figures, and constantly referred to in my later memoirs; and I shall presently mention an observation made last year by Dr Harless while at Triest, which affords a highly interesting proof of its existence. I need not add one word to shew the importance of an orifice in such a situation.

The ovum and its ovisac arise out of a cytoblast, Plate I., fig. 37. I must refer to my memoirs for the details of this change,* intending here to confine my remarks to the mature ovum.

The essential part of the ovum is the germinal vesicle or cell (the part not coloured in the figure last referred to, and *c* in fig. 45); and more particularly its nucleus,—the *germinal spot*. The first changes connected with fecundation, that are discernible in this spot in the ovum of the Rabbit, seem to be of a preparatory nature; for they begin *ante coïtum*, the animal being in the state of heat. They are such as those described in pp. 205–208, and represented in figs. 7 to 15. The germinal vesicle thus fills with cells in concentric order (fig. 45, *c*); and now the outer layer, liquefying, becomes a sort of pabulum for the second layer, which then occupies its place, and so on.

At length the ovum becomes fecundated, by the introduction of a substance from the seminal fluid into the hyaline centre of the germinal spot, which then passes to the centre of its vesicle or cell (fig. 16).† And now the contents of the cell, no longer in communication with the exterior by a parietal nucleus, undergo changes, having sole, or at least especial reference to that fecundated centre. The concentric layers of cells are successively and rapidly dissolved, and as speedily replaced by fresh broods of cells arising in the centre. It must be recollected too, that the cells in the concentric layers are themselves filled with other cells, undergoing like changes.

Fig. 16.



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* Phil. Trans., 1838, Plate V., figs. 1, 2, 3, 18, 19, 20, and Phil. Trans., 1841, Plate XXV., figs. 164 to 173.

† This figure, and several which follow it, are in outline only.

After this assimilative process has continued for a sufficient length of time, that which has become the nucleus divides into two halves (fig. 17); which two halves, by appropriating the result of this cell-formation, and by the same process as that above described, are enabled (fig. 18) to give origin to two cells, intended to succeed the parent. (For each of the two halves just mentioned is a globule of hyaline, such as the largest of those in the woodcut fig. 1; gradually becoming (figs. 2, 3) a cytoblast (fig. 4), and then a cell (fig. 5); and the nucleus of this cell presents a repetition of the same process as that bringing successively into view, figs. 6 to 15; which should be rapidly glanced over in their numerical order, the eye being directed to the ever active central hyaline.)[*]

Fig. 17.

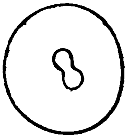
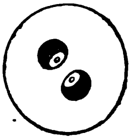
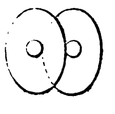


Fig. 18.



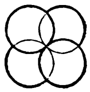
The mysterious hyaline centre of a nucleus, that is to say, the centre of the germinal spot, exercising an attractive force, is thus the point of fecundation in the ovum; and assimilation having been effected, and division of the nucleus accomplished, and the membrane of the germinal vesicle, as well as all the transitory cells, having disappeared by liquefaction, two cells remain (fig. 19), the foundation of the new being.

Fig. 19.



Of the further changes induced by fecundation, the following is an outline. After cleavage, the nucleus of each twin cell (fig. 19), gives origin to two other cells (the membranes of the parent-cells undergoing liquefaction), so that the foundation of the new being now consists of four cells, fig. 20. After cleavage of the nuclei of these four, there arise, in like manner, eight (fig. 21); and a continuation

Fig. 20.



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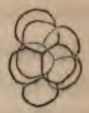
[*] During these changes, the germinal vesicle enlarges at the expense of the so-called yelk, until it comes to occupy almost the entire ovum.

of the same process effects a doubling in the number of the cells, until a mulberry-like body has arisen, consisting entirely of cells; in the centre of which mulberry is found a cell having the same general appearance as the rest, but of larger size,—a sort of queen bee in the hive. *The nucleus of this larger cell is the rudimental embryo.* And in the development of this nucleus into the embryo, the first discernible changes are of the same kind as those exhibited by the nucleus of every cell subsequently developed from it, and entering into the formation of the embryo.

Fig. 21.



From what I have already stated, it will be seen that the twin cells in fig. 19 are endowed with qualities resulting from the fecundation of the germinal vesicle, their parent-cell; such endowment of the young cells being referable to self-division, effected by a process of assimilation.

The reproduction of the cell is thus essentially fissiparous, its contents undergoing division after having been assimilated to the nucleus; which nucleus, therefore, it is that is reproduced,—this nucleus being a portion of the remarkable substance, hyaline, the reproduction of, or assimilation by, which seems to be a main purpose for which cells are formed.

Briefly recapitulating,—the hyaline, by a very elaborate process, assimilates new matter, and is reproduced by division. Through successive divisions of this substance, properties descend from cell to cell,—new properties being continually acquired as new influences are applied; but the original constitution of the hyaline not being lost. (And why do nuclei continue for a while at the surface of their cells, but for a purpose analogous to that for which the germinal spot or nucleus continues, up to a certain period, at the surface of its cell in the ovum?)

I conceive that what is seen taking place *in the manner here referred to*,—first in the germinal vesicle, and then in the individual cells, the descendants of this vesicle,—is essential to what we observe in the reproduction of the entire organism, namely, a mysterious reappearance of the qualities of both parents in the offspring; manifesting itself, as this reappearance does, in the assemblage and metamorphoses of the cells.

It having been in the germinal spot of the ovum, the most important of all nuclei, that I first saw the remarkable process above described, and illustrated in figs. 8 to 15, I may hereafter refer to the changes the nucleus undergoes, by the expression, *germinal spot process*. But after that process had been seen, and for a long period minutely traced, it was impossible to prosecute researches on the ovum, without discerning more or less of it in the nuclei of various cells. I recognised it in every nucleus seen to descend from the germinal spot, and subsequently saw it in bodies which, not only in the embryo, but at all periods of life, have apparently descended from that spot—the corpuscles of the blood; giving, in proof of this, hundreds of drawings in my memoirs. And having seen it also in pus and mucus globules, and epithelium cells, as well as in the elements of all the tissues examined, including those of nearly every tissue in the body, I concluded the process in question to be universal; and have never since met with a cell, the interior of which led me, for a moment, to doubt the correctness of that conclusion.

Figs. 22, 23, 24, 25, representing blood-cells of the Sparrow and fœtal Ox, afford examples of the *result* also of the process in question being the same as in the ovum; the cell in fig. 25 being about to produce twin-cells. The process, however, may lead to a very different result. Thus, in Plate I., fig. 36, β, there are seen escaping, from a parent-cell, *many cytoblasts.**

Fig. 22.

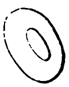
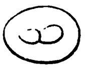


Fig. 24.



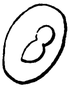


Fig. 23.

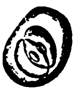


Fig. 25.

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* The cytoblasts at β in this figure are blood-discs of the fœtal Sheep. It will be seen that they are not round, like the blood-discs usually circulating in the Mammalia, but elliptical. The fact is that they are *young* blood-discs, having the same form as that which, according to my observations, is the first form of blood-discs in all animals. I saw it to be so at least in animals at both ends of the scale, including Man. I have given drawings of these blood-discs from the Tadpole, fig. 35, and Leech, fig. 36 α. There is a form, however, anterior

Descending, as cells thus do, from an original mother-cell, and this by cleavage of the nucleus of that mother-cell, and all subsequent nuclei being propagated in the same way, by fissiparous generation,—such being the process, every nucleus or particle of hyaline is a sort of *centre*, inheriting more or less of the properties of the original nucleus, the germinal spot of the fecundated ovum; and exercising an assimilative power. (The corpuscles of the blood are floating centres of assimilation.)

I wish it to be distinctly understood, that, in my researches, I always endeavoured to keep strictly to observation, avoiding all theory; and it may be here remarked, that some of those on the divisions in the ovum, just referred to, were made before I knew much of cells. Of one kind of animal alone, the Rabbit, about 150 were sacrificed in these observations; some of the most important of which I had the opportunity of enabling my honoured friend Professor Owen to confirm, by submitting the objects under the microscope to his practised and most rigorously scrutinizing eye.

How satisfactory now to find my observations confirmed by researches subsequently made by others. Thus the younger of the brothers Goodsir has since found, in the ovum of a cystic entozoon,* the germinal vesicle to fill with cells from the germinal spot, by a process obviously the same as that which I had described in the ovum of the Mammal; the germinal vesicle enlarging so as to occupy the entire ovum. Compare in Plate I., figs. 45 and 46; and read the description of the latter figure.

Farther, how satisfactory now to find those observations of mine adopted and formally applied in various departments

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to that of the elliptical disc, and, of course, quite as general; viz., the globular, for every disc (cytoblast) appears to have once been a pellucid globule of hyaline.—No observer can learn the structure of the blood-corpuscles, who does not carefully investigate their mode of origin; and this, not in blood taken from large vessels, which are merely channels for conveying it, but in that contained, and almost at rest, in the capillaries, and especially in the capillary plexuses and dilatations.

* *Cœnurus cerebralis.* Trans. Royal Society, Edinburgh. 1844. Vol. xv., Pl. *xvi.*

of physiology; for instance, by Professor Goodsir, who thereby makes a plain pathway through some of its most hidden regions.* Thus for example it is that, regarding every nucleus in his germinal membrane (Bowman's basement membrane) as a germinal spot,—and keeping in view what he denominates "that most important observation of Martin Barry on the function of the nucleus in cellular development,"—he is enabled to shew, that epithelium cells have their origin in the *nuclei* of that membrane,—an acknowledgment to me the more valuable, from the fact, discovered by Goodsir, in the ducts of glands, that the epithelium cell is the secreting organ.

This brings me to notice a chapter by the same author, entitled "*Centres of Nutrition*," regarding which he writes as follows:—"The phenomena presented by these centres incline me to regard them as destined to draw from the capillary vessels, or from other sources, the materials of nutrition, and to distribute them by development to each organ or texture after its kind. In this way, they are to be considered centres of germination; and I have elsewhere named them germinal spots, adopting the latter term from the embryologists. The centre of nutrition with which we are most familiar, is that from which the whole organism derives its origin—the germinal spot of the ovum. From this all the other centres are derived, either mediately or immediately; and in directions, numbers, and arrangements, which induce the configuration and structure of the being."†

That I can have no hesitation in admitting all the attractive force here attributed by Professor Goodsir to those centres, is obvious from its having been myself, I suppose, who made known the fact, that "the whole organism derives its origin from the germinal spot of the ovum;" and from what I wrote six years ago on the subject of centres in general. Thus, in the Philosophical Transactions for 1841, p. 201, will be found the following words, already quoted,—resulting, it will be seen, from my conviction that the germinal spot process, described by myself, was the same in all cells. I said, "It will be remarkable if the mammiferous ovum, which, because of its minuteness and the difficulty of obtaining it, had been considered beyond the reach of satisfactory observation, should now become the means of studying, not merely other ova, but certain processes by which nourishment is communicated, and the growth of the body effected at all future periods of life. Such, however, I think will really be the case."

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* *Anatomical* and Pathological Observations by John Goodsir, F.R.S.E., &c. *and H. D. S. Goodsir*, M.W.S. 1845.

† *Loc. cit., p.* 1.

As affording probability that this anticipation would be realized, I figured *centres*, *special centres*, of reproduction and assimilation; directing attention more particularly to such centres as were situated in the walls of tubes,—as, for instance, in those entering into the formation of muscle (fig. 26), the crystalline lens (fig. 27), and nerve: on which I remarked, "We may hereafter see reason for thinking it not unimportant that the contents of the 'primitive' cell and those of the 'secondary' cylinder should have their origin in the nucleus."* And subsequently, when those centres were referred to, my views regarding them were stated in the following words:—"I conceive them to be *centres* of assimilation; having been led to this opinion by observing, in the first place, that they present the remarkable orifice in question; and, secondly, that they are reproduced by self-division. They descend in this manner from the nuclei of the original cells of development, *i.e.* from the nuclei of the corpuscles of the blood. That they are the source of new substance is very obvious; for they may be seen either unwinding into a filament, or becoming spindle-shaped to form one. But what I wish to

Fig. 26.

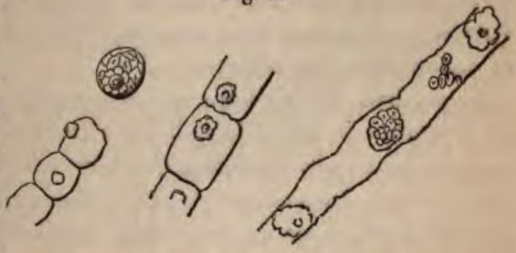
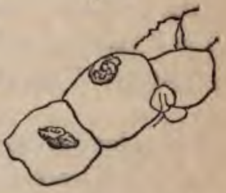


Fig. 27.



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* *Phil. Trans.*, 1841, p. 236.

add is, that the origin of new filaments in these nu appears to me to have especial reference to that assimilati of which they seem to be the centres. Although eve nucleus seems to possess a reproductive property, there thus special centres of reproduction. Such centres were al particularly indicated in one of my former memoirs,* as ex isting in the epithelium, the pigmentum nigrum, 'cellula tissue, and cartilage. In describing the first origin of muscl nerve, and the crystalline lens, also, I directed attention t such centres, stating that we might hereafter see reason fo thinking it not unimportant that the contents of the 'primitive' cell, and those of the 'secondary' cylinder, should have their origin in the nucleus; and I was particularly desirous of connecting this fact with the existence of the orifice in question. It will now be seen that I had in view the subject of *assimilation* now referred to."†

Such, then, having been my views long since, I can have no hesitation in admitting all the attractive force attributed to those centres by Professor Goodsir; whose theory of nutrition, indeed, harmonizes so fully therewith, that a friend pointed it out to me as the formal application of my views. And on a perusal of the important volume just referred to, by the brothers Goodsir, I have the satisfaction to find, that all in it relating to the "function of the nuclèus in cellular development," appears to be derived from the same source.

I have here great pleasure in acknowledging that Professor Goodsir's discovery of the real structure of basement membrane, appears to me an important step towards a solution of the question—How do the corpuscles of the blood enter into the formation of structures arising on the "free surfaces of parts or organs"? That the blood-corpuscles *do* enter into the formation of such structures, I must take to myself the credit of having shewn, by tracing them into the incipient chorion in the Fallopian tube.‡ But it is now possible, I think, to offer an explanation of the mode in which they are

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[*] Phil. Trans., 1841, par. 119, 120, 135, 144.

[†] Edinburgh New Philosophical Journal, October 1, 1843, pp. 207, 208.

[‡] On *the Corpuscles of the Blood*, Phil. Trans. 1840.

made to do so; which will be found at the end of this paper. I would here merely add that the discovery now referred to, is that of *nuclei* found by Goodsir in the basement membrane, the presence of which nuclei induced him to call that membrane a *germinal* membrane; for, adopting my views on the "function of the nucleus," he believes the nuclei in his germinal membrane to absorb from the capillaries on one side of that membrane, and give origin to the epithelium-cells on the other.

Here I must not omit to say, the orifice I have so generally met with in the nucleus, is most important in the function of absorption by that body, especially in centres such as those present in germinal membranes, fig. 50, and in the tubes, figs. 26 and 27; that orifice being in cells, as well as in certain of the Infusoria, a sort of mouth for nutrition; and in all it is equally an opening for fecundation, or what is equivalent to fecundation, for the introduction of a substance to be subjected to that assimilative process which prepares the cell for either reproduction or any other purpose. In tissue-tubes, such as those in figs. 26 and 27, the nucleus with its orifice seems to remain for the purpose of exercising an appropriative power,—forming muscle in one instance, fibres of the lens in another, and so on.

Of the existence of the orifice in question, we have, I think, a highly interesting proof in observations made last year by Dr Harless;* who, in examining cells of the ganglion-globules in the lobus electricus of the Torpedo Galvanii, found nervous filaments connected with what he terms the nucleus of an inner cell. I have copied three of his figures; and if these be referred to, Plate I, fig. 44, it will be found that his nucleus of the inner cell is my orifice, situated in α and β in a hyaline nucleus, membranous at the surface,† and in γ in a finely granular nucleus or cytoblast,—being in the latter double. The nervous filaments so connected, and, as Dr Harless says, *proceeding from* the point in question, he describes as pellucid, and as belonging to the medulla of the primitive fibres. In some instances, he saw two such filaments connected with cells in this manner (see β in fig. 44),—the directions taken by the two filaments having been so different, that he suggests whether one of them may not have belonged to a cerebral fibre, and the other to a peripheral nerve.

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* Müller's Archiv, 1846, No. III., p. 283.

† The "hollow nucleus" of Schwann.

From all that I have written concerning the nucleus of the cell,—which had been supposed to remain inert in the cell-wall, or to be "cast off as useless and absorbed,"—and especially from what I have said regarding the mysterious centre of that nucleus—the primogenital, absorbing, assimilating, formative, dividing, subdividing, even vibratory, and ever active hyaline—it will be seen that to me Dr Harless's observation must be one of surpassing interest. I refrain, however, from further remarking on it; the subject having been here referred to merely for the purpose of shewing that, by the existence of a connection between the nucleolus of the cell and nerve, that of the orifice in question is implied.

In connection with Professor Goodsir's observations on the placenta, I refer to my own, previously published, on the mode of origin of the chorion (Plate II., fig. 54 x); for the purpose of pointing out in the nuclei of the cells which enter into the formation of the chorion, the source of the germinal spots or centres figured by Goodsir in advanced stages of that membrane. (I would also direct attention particularly to a fact above referred to, the presence of nuclei or germinal centres in cell-membranes however small at first,—as for instance in the membrane of the ovisac. This I think not undeserving of remembrance, in connection with the subject of the enlargement which some cells undergo in both healthy and morbid growths.)

In his important paper on secretion, published in 1842,* Professor Goodsir recognises cells in general as endowed with a property which, I must do myself the justice to say, had not been overlooked by me when examining such cells as came under my notice. Thus, the year before, I had shewn the pigmentum nigrum to have its origin in cells. But it is evident Professor Goodsir was not aware of what I had written, or he would not have supposed the secretion contained in the cell-cavity to be produced by the cell-*wall;* for in a subsequent paper, in which he attributes the property of secretion to the *nucleus* of the cell, he acknowledges it to have been my observations "*on the function of the nucleus in cellular development*" that convinced him he had been in error in attributing that property to the cell-*wall*. So minutely had the mode of origin of the red colouring matter and that of the pigmentum nigrum been investigated by me, that I had found, in the first place, the red colouring matter to be formed by the hyaline nucleus of the cell; and, secondly, that red colouring matter to afford a medium for the formation of the black pigment, which also was seen to arise out of the hyaline nucleus within the cavity of the cell.

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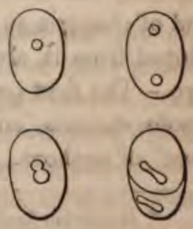
* Trans. R. S. E., 1842.

As all the tissues have their origin in corpuscles presenting the same colour, form, and size, as corpuscles of the blood,—and if all cells are derived, as I believe them to be, from the corpuscles of the blood,—it will be seen that the red colouring matter is a secretion of importance, and this on two accounts. In the first place, it is universal,—for everywhere, in greater or less quantity, it is reproduced; and secondly, it no doubt affords a medium for the formation of other substances in the cell-cavity, as well as for that of the pigmentum nigrum. *It corresponds in fact to the finely granular substance of every nucleus and cytoblast, which comes into view as new matter is appropriated by the hyaline.*

Figs. 28, 29, 30, 31.



I observe confirmations in other quarters also. Some of these were referred to on former occasions,* and need not be particularly mentioned here. They are, Schwann's delineations of the cells of cartilage, figures by Valentin of ganglion-globules, and drawings by Müller of the cells of morbid growths. Figs. 28, 29, 30, and 31, since published by Dr Bagge,† represent stages of the ovum of an intestinal worm; concerning which Professor Owen in a lecture remarked, "There is a close and interesting analogy between the above phenomena, which were published in 1841, and some of those communicated by Dr M. Barry to the Royal Society, in January 1841, and published in the Philosophical Transactions of the same year. The clear central nucleus of the blood-corpuscle [see figs. 22, 23, 24, 25] is there shewn to form two discs, which give origin to two cells. We may, likewise, discern in the pellucid nucleus of the yolk, dividing and giving origin to two yolk-cells, according to the German author, the hyaline nucleus of Dr M. Barry."*

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* Phil. Trans., 1840, p. 552, note. 1841, pp. 207, 208.

† De Evolutione *Strongyli*, &c. 4to. 1841.

Between the appearances presented by the mammiferous germ during the passage of the ovum through the oviduct and certain infusoria, including the *volvox globator* as figured by Ehrenberg (some of whose observations have been confirmed and extended by myself†), the resemblance, first mentioned by Professor Owen, is so remarkable that we cannot avoid the belief, that the same process operates in both. And farther, we have here a most interesting confirmation of the view, that the germ of the highest animals at certain periods, represents, or passes through forms permanent in the lowest.

Cilia-bearing cylinders arise, not by coalescence—as supposed by Valentin—but by division, like some of the Vorticellæ; which they resemble also in the position of the cilia.

I have already said that in Mammals, the rudimental embryo is no other than the nucleus of a cell; which nucleus originally has the same appearance and undergoes the same kind of changes as the nucleus of every cell subsequently developed from it, and entering into the formation of the embryo.—The first portion of the embryo that is formed is the *chorda dorsalis*, corresponding to the supposed "primitive trace" of authors. I have published a memoir‡ shewing this

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[*] Hunterian Lectures. 1843. P. 78.

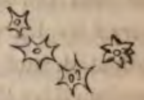
[†] See the Number of this Journal for October 1843, pp. 214–219.

[‡] Philosophical Transactions. 1841.

to be the case; and here also Professor Goodsir informs me that he adopts my views.

It may be added, that at a recent meeting of this Society the same talented observer mentioned an observation of his own on some of the poligastric Infusoria, as fully confirming mine,—that the nucleus sometimes passes directly into a spiral form while still within its cell. And in the same quarter my observation has been confirmed,—that the spiral is the most primitive of forms. (To this, I have no doubt, is referable the spiral form retained through life by some of the lower animals:—for instance, the spiral-shelled Mollusca.)

Fig. 32.



In the blood of animals which had been kept for periods varying from 18 hours to two days after death, I found the nuclei of the corpuscles dividing into segments (fig. 32),* and the segments sending out processes; which processes, becoming acuminated, passed into cilia. The vibrations of these cilia gave to the now star-like corpuscles a locomotive power. This I have observed to be precisely the mode of origin of the mammiferous Spermatozoa,—each of which is represented by one of the segments in that figure. (The segments and the cilia, it will be seen, are processes from the hyaline. Compare with fig. 8.)

I believe that when animal and vegetable substances are macerated in water, particles of hyaline (fig. 1), which are the true cell-germs, may assume an independent life in the form of Infusoria; and thus go far to explain the so-called spontaneous generation.†

It may be added, as an observation I have not published before, that the markings on the egg-shells of Birds afford a beautiful display of colouring matter, obviously formed in cells, and there seen on a scale so large as to be visible without a magnifying power; the colouring matter being sometimes of deepest hue in what had been the situation of the nucleus, and presenting zones, which grow paler and paler as they extend over a larger surface. These markings on the shells of eggs often present a curious resemblance to the ramifications of the pigmentum nigrum, proceeding from reproductive *centres;* as in eggs of several of the Buntings.

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* Phil Trans., 1841, Plate XXII., Fig. 105, γ.

† On this subject see my views more at length, in No. lxx. of this Journal (October 1843), pp. 219, 220.

Fig. 52 in Plate II. represents part of an ovum of the Common Leech. I would direct attention to the resemblance between it and what I believe to be corresponding parts in ova of the Rabbit, figured by myself in the Phil. Trans. for 1839, Plates VI. and VII., fig. 114, &c., *bb*, *bb'*, and *a.p.*

The long pellucid area *a m* in fig. 52 is the situation of the future nervous ganglia in the Leech. Compare it with an obviously corresponding area in my figures just referred to—the situation of the future spinal cord. Again, the round iris-like body at *a*, fig. 52,—so far in advance of the other parts in its formation,—is the sucking apparatus of the Leech, endowed apparently at this early period with suctorial power. Compare it with the nucleus of my queen bee cell in the same and preceding figures; which nucleus I have always maintained to be the rudimental embryo in the mammiferous ovum.

But the figure from the ovum of the Leech was published by Professor E. H. Weber in Meckel's Archiv, in 1828,—many years before any thing was known of the physiology of cells. It serves as an example of the importance of fidelity in delineation; for I do not think that now, with all our knowledge regarding cells, we could give a drawing much more satisfactorily representing them;—at least on the same scale.

That ovum of the Leech, elliptical in form, had a length not exceeding three Paris lines. Yet even at this early period, Professor Weber noticed that the sucking property of the iris-like body, *a*, had begun to manifest itself in alternate contraction and dilatation of the central orifice; the effect of which seemed also visible in the surrounding albumen. Compare these facts with observations recorded by myself on the nucleus, *bb*, in the mammiferous ovum; remembering my orifice in the nucleus of the cell,—and all that I have now said, as well as that which is implied by what I had previously written, on an attractive force or suctorial power as exercised by that nucleus; for assuredly no other than what had been such a nucleus is to be recognised in Weber's figure.

Professor Goodsir finds his "germinal membrane," met with on the free surfaces of parts or organs, to consist of "cells, with their cavities flattened, so that their walls form the membrane by cohering at their edges."*

I am happy to have it in my power to confirm this observation, by stating that the description now quoted fully applies to two membranes in the mammiferous ovum seen by myself, and figured in the Philosophical Transactions for 1839.† One of these, my "amnion," corresponds to what had long been called the "germinal membrane" in the ovum of the Bird: and the other I stated to be the foundation of the lamina subsequently vascular. Several figures of the latter I have copied on this occasion,—Plate I., figs. 33, 34; and I recommend an attentive perusal of the description of these figures. Tubes such as those entering into the formation of muscle, fig. 26, and the lens, fig. 27, are certainly germinal membranes; appearing thus to have the power of absorbing for nutrition. And if fig. 38 in Plate I. be referred to, it will be found that the membrane of my ovisac presents a structure of the same kind; which is interesting, 1. from the subsequent ramification of numerous capillaries on it, by which there is formed a Graafian vesicle,‡ and, 2. from its inner surface being the situation of the "membrana granulosa," a sort of epithelium. I long since shewed the incipient chorion to have a like structure (Plate II., fig. 54, *x*); and, as already said, there is good reason for believing it to be the structure of other membranes.§

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* Loc. cit., p. 3,

† Plates VII. and VIII.

‡ Of which the ovisac is now the inner membrane. Phil. Trans., 1838, p. 311.

§ While these pages are passing through the press, I learn that Professor Harting of Utrecht, and M. Mulder, have lately found the primitive membrane of the vegetable cell, even when young and before its thickening, to present many pores. This seems to be in perfect keeping with what I long since shewed to be the mode of origin and structure of the membrane of the ovisac, already

I had also observed and sketched, as seen both in the Fallopian tube and at its fimbriated éxtremity, traces of what was probably a corresponding membrane, though perhaps in an incipient state (Plate II., fig. 48, γ) ; in one instance along with young and blood-red epithelium cells. The latter when fully formed, I had also seen and figured (Plate II., fig. 54, ζ) "adhering by their distal extremities," in the manner described by Goodsir; as well as what appear to have been cytoblasts and cells of a germinal membrane, for forming epithelia, fig. 54, $\alpha, \beta, \gamma, \delta$. And I would offer the diagram, fig. 53, in Plate II., as the probable mode of origin and evolution of the epithelium cells.

This diagram represents a germinal membrane, *resting on capillaries such as those in fig.* 48, β, *Plate II. ;* having on the extreme left a cytoblast, marked IV., in the state represented by the woodcut, fig. 4. The cytoblast becomes a cell, marked V., which is in the state of that in the woodcut, fig. 5. VI. In the diagram corresponds to the woodcut, fig. 6; and so on, as far as XI.; after which the epithelium cells are given off, adhering to the membrane of the cell or cells in which they arose ; and leaving the nuclei, which are in outline only, in a state for producing another brood.

This explanation, rendering it needless to suppose that there occurs a splitting of the germinal membrane into two layers, seems to meet a difficulty spoken of by Goodsir; and it probably applies as much to the supposed splitting of what has been called the "germinal membrane" in the ovum; for in both there is formed a "mucous" or epithelial surface.

But if the diagram in question explains the mode of origin of epithelium cells, it may also admit of a further and a more important application. *I found the red colouring matter of*

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mentioned in pp. 207 and 209 of this paper, and to accord with my belief in the compound structure of the membrane of every cell. With regard to the question at present much occupying the attention of Vegetable Physiologists—Whether, in the thickening of the cell-membrane, the inner or the outer layer is the last formed,—all my observations on the functions of the nucleus are of course in favour of the *inner* layer being formed last (see fig. 40.) But if the primitive membrane of the cell be, as I believe, a compound structure, it has in its own substance the means of contributing towards its thickening. (See Phil. Trans., 1841, Plate XXV., figs. 170–173.)

the blood to reappear in the epithelium cells. And I am incapable of understanding the presence in large quantity, in the Fallopian tube, of red blood-discs and cells such as those at θ in the figure, without supposing that they have here, as well as elsewhere, their origin in parent-cells, such as the epithelium cells at η seem to have been. Now, out of blood-red discs (fig. 54, θ), as I long since shewed, are formed cells with processes or arms (ι), and out of these is formed the chorion (ϰ); which, by the remarks now made, I am endeavouring to connect with the blood-corpuscles in the circulation,—for, it will be recollected, the epithelium cells derive their origin from capillaries such as those in fig. 48, β, *containing only blood-corpuscles, and no liquor sanguinis at all.*

And if it be thus possible to trace the elements of the chorion from a free surface, through epithelium cells, to the blood-corpuscles, how much more is it not possible to refer to those corpuscles the elements of the tissues, as well as all nutrition; seeing that here we have a more direct communication, and no epithelium cells?

Having, six years since, investigated the mode of origin of nearly every tissue of the body, and written a long memoir for the sole purpose of communicating the results of those investigations,* I need not here refer to the subject in any other, than a very general manner. In that memoir I stated the tissues to be formed of "corpuscles, having the same appearance as corpuscles of the blood," and did not hesitate to add my belief that the corpuscles having that appearance, and entering into the formation of the tissues, were derived from blood-corpuscles which had been in the circulation. My views are stated somewhat in detail, in the description of figs. 47 and 48 of Plate II.; to which description I refer, and would here simply add, that there was no liquor sanguinis in the capillaries represented in those figures; that I believe it to be the hyaline from the centres of blood-corpuscles that escapes from the capillaries for nutrition and the formation of new parts,—globules of which hyaline become red cytoblasts, exactly like those still in the circulation; and that, centres being observable in the walls of capillaries (Plate II., fig. 49 δ,) which are evidently nuclei of the cells forming these capillaries, it is probable that *the walls of capillaries also are germinal membranes*, which, by their nucleal orifices, take up and distribute on their exterior the hyaline of the blood-corpuscles; this hyaline being then absorbed by other centres for nutrition or the formation of new parts.

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* "On the Corpuscles of the Blood," Part III., Phil. Trans. 1841.

Explanation of the Plates.

PLATE I.

Fig. 33. From ova of the Rabbit. α, Network representing part of the incipient umbilical vesicle. A hollow process or little sac, consisting of incipient cells, having been given off by the rudimental embryo (the nucleus of a cell), those cells coalesce where in contact, and form this network; which, enlarging, at length lines the ovum. The pellucid nuclei of the coalesced cells are at the surface of their cells. They are surrounded by minute globules, and *have no nucleolus*. β, The same object in a stage more advanced. It now lines the ovum, and the membranous portion of the network is disappearing. *A nucleolus is now coming into view.* γ, Stage still more advanced;—all traces of a network have disappeared, and the nuclei alone remain.—(Phil. Trans., 1839, Plates VIII. and VII., Figs. 132, 120, 121.)

Fig. 34. A later stage of the objects in the preceding figure. Cells have been formed by cytoblasts, arising out of bodies such as those in fig. 33, γ. Each cell has a pellucid nucleus, corresponding to that which was the nucleolus in fig. 33, γ. And *what were nucleoli in fig.* 33, γ, *are now nuclei*. Of these nuclei, there are seen in the figure three stages, α, β, γ,—α exhibiting the least advanced, and γ the most forward stage. α, The nucleus is merely a portion of hyaline, not surrounded by any globules, *and not having a nucleolus*. β, Globules are now seen surrounding the nucleus, and in two instances a nucleolus is coming into view. γ, The nucleus is now surrounded by a membrane, formed by globules such as those at β, and it *has now a well-defined nucleolus*. —(Phil. Trans., 1839, Plate VIII., fig. 150.)

Fig. 35. Young blood-corpuscles of the tadpole of the large Toad of Jersey, They are cytoblasts.—(Phil. Trans., 1841, Plate XX., fig. 74, β.)

Fig. 36. Young blood-corpuscles, α of the Leech, and β of the fœtal Sheep. Some of the latter are escaping from the parent-cell.—(Phil. Trans., 1841, Plates XIX. and XXIII.)

Fig. 37. Blood-red bodies derived from blood-corpuscles of a bird, the Wryneck (*Yunx Torquilla*, LINN.); and forming ova in their ovisacs. α, Originally elliptical, has become round. β, The outer part consists of cytoblasts, which surround a central cytoblast. γ, The central cytoblast has become a cell, the germinal vesicle, and exhibits a distinct orifice in the situation of the future germinal spot. (The entire body γ appeared to be membranous at its surface, from the remains of the cell-membrane formed by its cytoblast.) δ, The germinal vesicle and its orifice are much larger. ε, The germinal vesicle has further increased in size, and the peripheral cytoblasts are dividing into minuter cytoblasts, *i. e.*, they have formed cells which exhibit (are being reproduced by) a cell-formation in their interior. (Phil. Trans. 1841, Plate XXV., figs. 165–167.)

Fig. 38. From the same ovary. Portion of the membrane of an ovisac, which was elliptical, and $\frac{1}{3}'''$ in length as it lay crushed under glass. It still presented a pale tinge of red, as well as a trace of the minute cells of which the membrane is composed. (Phil. Trans., 1841, Plate XXV., fig. 172.)

Fig. 39. Blood-corpuscles, and cells &c. derived from blood-corpuscles, of the Lobster, Tadpole, Cod, Chick *in ovo*, Rabbit, and fœtal Sheep and Ox. All drawn on the same scale; magnified 600 diameters. (Phil. Trans., 1840 and 1841.) Most of the larger of these bodies are very much in outline.

Fig. 40. The germinal vesicle, and some of the surrounding substance, in an ovum of the Tiger, after the ovary had been for a short time macerated. The germinal vesicle had become elliptical, and out of the germinal spot there had arisen three concentric cells. (Phil. Trans., 1839, Plate V., fig. 89). See the description of fig. 41.

Fig. 41. A nucleus in its cell. The nucleus consists of two concentric cytoblasts, the inner being the one last formed. The nucleus of this cell was the rudimental embryo of the Rabbit. (Phil. Trans., 1839, Plate VIII., fig. 148.)

Fig. 42. The nucleus of one of the twin cells in an ovum of the Rabbit of 17 hours, from the Fallopian tube. Its external part consisted almost entirely of cytoblasts. (Phil. Trans., 1840, Plate XXIV., fig. 192.)

Fig. 43. Nucleus of one of the transitory cells in an ovum of the Rabbit from the Fallopian tube, of the same age as that in the preceding figure. (Phil. Trans., 1840, Plate XXIV., fig. 189.)

Fig. 44. After Harless. α, β, γ, Cells of ganglion-globules in the lobus electricus of the Torpedo Galvanii; shewing a connection between nervous filaments, and what Dr Harless terms the nucleus of an inner cell. See pp. 217, 218 of this Journal.

Fig. 45. α, An ovarian ovum of a rabbit killed in the state of heat; in diameter $\frac{1}{11}'''$. *c*, Germinal vesicle. β, The pellucid centre of the altered germinal spot on a larger scale, from an ovum of the same ovary as the ovum α. (Phil. Trans. 1840, Plate XXII., Fig. 162.) Compare with Fig. 46.

Fig. 46. After H. D. S. Goodsir. Ovules of Cænurus cerebralis. α, "Fifth stage of ovule. C, primary germinal vesicle; D, primary circle of cells; E, central cell of primary circle of cells become larger; F, its nucleus, become larger and nodulated." β, "Ovule of Cænurus, very far advanced in the discoidal period of developement. The concentric circlets of cells are seen; and the central circlet near to the lower edge as it was pressed between the plates of glass, shewing that there is elevation, to a certain extent, during the latter stages of this period. A, external covering of gemmule; C, some of the concentric circles of young cells; C, central cell of last formed circlet; D, its nucleus and clear space." (Trans. R. S. E., 1844, Vol. XV., Plate XVI., Figs. 5 and 9.)

PLATE II.

Fig. 46½. Blood-corpuscles from the edge, posterior surface, and neighbourhood of the crystalline lens in the chick *in ovo* and a fœtal sheep; very much enlarged, especially the hyaline, which has increased in quantity at the expense of the red colouring matter. Magnified 600 diameters. (Phil. Trans., 1841.) See the description of Fig. 47.

Fig. 47. A small portion of one of the dilated capillaries at the edge of the crystalline lens in a fœtal ox of 18 inches; filled with blood-corpuscles in greatly enlarged and altered states. This capillary was most unequal in calibre at different parts. Magnified 600 diameters. (Phil. Trans., 1841, Plate XXIV., Fig. 152.) The blood-corpuscles, or rather cells, were for the most part ruptured. At the upper part of the figure on the right hand, they were still entire. The part reddened is the red colouring matter, and the white exhibits the hyaline from the ruptured cells. In the capillaries the red colouring matter of the corpuscles serves as a sort of pabulum for the hyaline centres of those corpuscles; the quantity of which hyaline in some corpuscles becomes, as in the present instance, where new parts were forming, prodigiously large; the red colouring matter in proportion disappearing. This capillary *contained no liquor sanguinis*. The hyaline, from the nuclei of the blood-cells, is the substance which seems to pass through the walls of the capillaries; and in some parts (α) there are seen what appear to be nucleal orifices for this purpose. (See also δ of Fig. 49.)

Globules of the hyaline, after escaping from the capillaries, seem to become cytoblasts, in which red colouring matter is again formed; not excepting even those entering into the formation of the pellucid crystalline. Several of the bodies last referred to are seen in outline at β. (This originally colourless substance, derived from the nuclei of blood-cells, appears to constitute the essential part of coagulable lymph, to organise the same, to serve for nutrition, and to give origin to the tissues, &c. It seems to be this same originally colourless substance, derived from the nuclei of blood-cells, that forms the exudation-corpuscles of authors, the filaments of false membrane, and the filaments in coagulating blood; filaments which, as I have shewn, here and there arise while this substance is still within the cells.)

Fig. 48. α, Outline of capillaries as seen at the inner surface of the wall of the infundibulum of the Fallopian tube, in a rabbit killed 5½ hours *post coïtum*. At the lower part are epithelium cells carrying cilia. These epithelium cells are arranged parallel to one another, and perpendicular to the general direction of the capillaries. The small extremity of the epithelium cells is directed towards the capillaries: whence their frill-like appearance (in the figure), and the great extent of their ciliary surface. External diameter of the largest vessel $\frac{1}{200}$.''' 100 Diam. β, A portion of the capillary plexus α, magnified 300 diameters. These capillaries are in the state of vital turgescence; *filled* with blood-corpuscles (which have become true cells), *to the exclusion of surrounding fluid*. Such is the state of all the capillaries in that region at such time. Many of the corpuscles so minute that their hyaline presented the appearance of a dark point. γ, Network formed by the coalescence of cells derived from corpuscles of the blood; which, like those entering into the formation of the chorion, have for this purpose sent out processes or arms. The finest ramifications in arborescent states of the capillaries, like that at α, have sometimes appeared to pass into a network of this kind.

Fig. 49. Capillaries in the course of formation by cells derived from corpuscles of the blood, in the retina of a fœtal ox of 18 inches. These cells, while still red (α), apply themselves together, so as to form a necklace-like object, composed of elliptical beads; and having coalesced, and become pale (β), and the membranous partitions having disappeared, they form a tube. Magnified 500 diameters.—(Phil. Trans. 1841, Plate XXII., fig. 107). Nucleal centres or orifices, forming communications apparently between the interior and exterior of these vessels, are seen at δ in two parts.

Fig. 50, after Professor Goodsir (Anatomical and Pathological Observations, 1845, Plate I., fig. 11). "Diagram of mucous membrane of jejunum when absorption is not going on. *a*, Protective epithelium of a villus, *b* secreting epithelium of a follicle, *c c c* primary membrane, with its germinal spots or nuclei *d d*. *e*, Germs of absorbent vessels. *f*, Vessels and lacteals of villus."

Fig. 51. From the Ox. Granules of the ovisac with their nucleus. In one instance a single granule presents two nuclei. This granule measured $\frac{1}{150}$th Paris line in length.—(Phil. Trans., 1838, Plate VIII., fig. 73.) These granules are cytoblasts; exceedingly well seen in the ovisac: which contains little of any thing besides, in a solid form.—(See the four Plates, V., VI., VII., VIII., in the Phil. Trans, above referred to.)

Fig. 52. Part of the ovum of the Common Leech, drawn by Professor E. H. Weber, of Leipzig (Ueber die Entwickelung des medicinischen Blutegels, in Meckel's Archiv, 1828, Taf. X., fig. 7). See page 222 of this Journal.

Fig. 53. Diagram to shew the probable mode of origin of epithelium cells. (See p. 224).

Fig. 54. From the Fallopian tube of the Rabbit. α, β, γ, δ, Bodies which seem to have been the nuclei, cytoblasts, or cells, from a germinal membrane such as that in the diagram, fig. 53. (Compare δ in this figure with X. in fig. 53). ε, Young epithelium cells. (Compare with XI. in fig. 53). ζ, Fully formed epithelium cells, with their cilia. η, Cells filled with cytoblasts. They seem to represent parent-cells, giving origin to such bodies as those at θ, which are obviously derived from corpuscles of the blood. ι, Some of the latter (θ), now become cells with processes or arms; which, interlacing, enter into the formation of κ, the incipient chorion.—(Phil. Trans., 1840, Plates XXVIII., XXIX.)

On the Precipitation of one Earth or Metallic Oxide by another from Saline Solutions. By JOHN DAVY, M.D., F.R.S., London and Edinburgh, Inspector-General of Army Hospitals. Communicated by the Author.

Though this, I believe to be a subject very deserving of inquiry, as likely to throw light on many things in the economy of Nature, and to lead to useful results applicable to chemical analysis, it appears hitherto to have received but little excepting incidental attention; at least I am not acquainted with any researches that have been instituted expressly on it.

In the communication I have now the honour to contribute, I shall do little more than enter on the inquiry, having neither the leisure nor means requisite to prosecute it far; my hope is that the few results I may be able to bring forward, with their applications, may induce others who are more favourably situated to engage in and extend the investigation.

If lime, whether in the form of hydrate or carbonate, be added to any salt of alumine in solution, the latter earth is thrown down, and a salt of lime formed; and, after a certain time, judging from the experiments I have made, and as is reasonable to infer *a priori*, if a sufficiency of lime be used, the whole of the alumine will be precipitated.

Magnesia and its carbonate appear to have the same effect on the salts of alumine.

If lime be added to a salt of magnesia, the effect varies according to the nature of the salt. In the instance of the sulphate, the lime separates the magnesia,—magnesia is thrown down and sulphate of lime formed. In the instances of the nitrate and muriate of magnesia, no change takes place. Nor is there any change when the carbonate of lime is substituted for the hydrate; even sulphate of magnesia, and carbonate of lime remain unaltered on admixture.

If lime be added to a salt of iron in solution, whether in the state of peroxide or protoxide, a precipitate is effected, the oxide is separated. A similar effect as to separation takes place, if the carbonate of lime be used. In this instance, in the case of the protoxide, a carbonate of iron is formed, which, acted on by atmospheric air, is rapidly decomposed, carbonic acid gas escaping, and hydrated peroxide of iron appearing.

Magnesia and the carbonate of magnesia appear to have a like effect on the salts of iron; and alumine likewise appears to have the same effect.

If lime be added to a solution of phosphate of lime in the nitric, muriatic, or acetic acid, the phosphate of lime is precipitated, as the acid, the solvent, is neutralised. When carbonate of lime is used, the result is modified according to the strength of the acid in which the phosphate of lime is dissolved. If it be very dilute, as when distilled vinegar is employed, and calcspar in fragments be introduced, so as to occasion a slow generation of carbonic acid, then little or no phosphate of lime will be precipitated; it will be kept in solution by the carbonic acid, and so long as the carbonic acid be retained; but will be thrown down as soon as the gas escapes, slowly and in part, for example, on exposure to the air,—rapidly and entirely on the application of heat, so as to expel the whole of the gas by ebullition.

The effect of magnesia, and of carbonate of magnesia, on phosphate of lime in solution is analogous.

The introducing phosphate of lime thus, when treating of earths, is not, I admit, perfectly exact; I have been induced to mention it, from certain resemblances which it bears to the earths in its properties, and from the important part it performs in organic nature,—a part depending on qualities differing but little from those of the earths.

Few and simple as these experiments are, the inferences from them and their applications are not without interest. If we consider what has been stated of alumine, how it is precipitated by lime and magnesia and their carbonates, ought we to expect to find it in solution in the waters of the ocean, or in any waters coming in contact with calcareous or magnesian rocks? Experience proves that we do not; alumine has never been detected in sea-water, or in any water exposed to the action of the earths mentioned. I may add, the circumstance that alumine has not the property of combining with and being rendered soluble by carbonic acid, seems to be wisely designed, in conjunction with the manner in which it is precipitated from its saline solutions, and in conjunction with its peculiar quality of retaining moisture and arresting water in its course; properties which render it fit specially for forming the basis of soils, and for entering into the fissures of rocks, and by closing them, giving origin to springs.

If we consider also what has been mentioned of the oxide of iron, as precipitated by the earths, and its relation to carbonic acid; does it not equally account for the absence of this substance in the waters of the sea, and in all waters exposed to the air, running over or resting on calcareous, magnesian, or aluminous rocks? Owing to the readiness with which it is thrown down from its saline solutions, it is easy to comprehend its being collected in nodules and layers, and abundantly associated, as it so often is, with earths fitted, when it is taken to be applied to the use of man, to serve as a flux in the fusion and reduction of its ores. Owing to the same circumstance, viz., the facility with which it is precipitated; whether by the action of lime, or magnesia, or alumine, separating it from an acid, or simply by the absorption of oxygen, and the conversion of the protoxide into the peroxide, and the disengagement of carbonic acid; it is not difficult for us to understand how this oxide, the peroxide, is the colouring matter almost universally of the earth's surface, imparting various tints of brown and yellow, admirably harmonizing with the colouring derived from plants, whether in its greatest purity, as on the surface of limestone rock, itself white, giving tints rivalling almost the hue and richness of gold, such as we see in the sea-cliffs and mountain precipices of calcareous countries, especially of Greece; or, in less purity mixed with different proportions of vegetable matter in a state of decay, giving to the soil a soberer and more suitable hue for an expanded surface in almost endless variety of shades of red and brown.

What has been stated of phosphate of lime seems very deserving of consideration, in connection with vegetable and perhaps animal physiology. Does not the precipitation of this phosphate, in the manner mentioned, from acids, tend to preserve a store of it on our lands? Were it otherwise, would not the greater part of it soon be washed into the ocean, and be lost to man? And then its solubility in water impregnated with a gaseous acid—the carbonic—always tending to pass off into the atmosphere, affords great facility to its being conveyed from the soil, and deposited where required in the structure of plants during their process of growth.

I have adverted to the solubility of phosphate of lime by the acetic acid, and even when this acid is much diluted, as in the instance of distilled vinegar. From some descriptions we have even in the works of respectable authors of the methods of examining soils, and of detecting this compound, this quality of the solubility of phosphate of lime in acetic acid, might be supposed not to exist. Thus, in an able work on Agricultural Chemistry, it is stated, under the head of detecting phosphate of lime in soils, that if the precipitate by ammonia obtained from a solution after the action of muriatic acid on a sample of soil, is not entirely dissolved by acetic acid or vinegar, the presence of phosphate of lime is indicated.* Now, this surely is an error, as phosphate of lime is soluble in acetic acid and in vinegar. This is so familiarly known that it hardly requires to be illustrated. It may not, however, be amiss to mention one or two results I have obtained. If phosphate of lime be digested in distilled vinegar, and the filtered solution be evaporated, the phosphate I find is recovered, as might be expected, unaltered, excepting in form. If the evaporation be carried on rather rapidly, the phosphate will be obtained in minute, thin, irregular plates possessing a good deal of lustre, and shewing a tendency to crystallization. If the process be carried on slowly, and with care, then the phosphate will be procured in the form of prisms, some of them distinctly hexahedral.

In commencing these observations, I expressed the belief that the precipitation of one earth by another, might be applicable not only to explain certain effects in Nature, some of which I have alluded to, but might also be made useful in chemical analysis. At present it would be premature to dwell on this part of the subject; I may hereafter recur to it. As an example I may notice the precipitation of alumine by lime and carbonate of lime, the non-precipitation of magnesia by these, excepting when in solution by means of sulphuric acid, and the precipitation of phosphate of lime by lime, but not by the carbonate, when so used, that the dilute solution, as in the instance of distilled vinegar, is capable of retaining sufficient carbonic acid to preserve the phosphate in state of solution.

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* Lectures on Agricultural Chemistry, by J. F. W. Johnston, F.R.S. Edition of 1844, p. 552.

In conclusion, I would observe that, as the earths are widely diffused, constituting, with oxide of iron, the greatest portion of the superficies of our globe, inquiries into their chemical action on each other can hardly be too minute or extended, feeling persuaded, as I do, that, if zealously prosecuted, these will not fail to throw light on many phenomena at present obscure, and to make us acquainted with the causes of series of changes in geology, as to which at present we have little more than conjecture.

BARBADOES, *March* 22, 1847.

On the Origin of Continents. By JAMES D. DANA.*

In a paper on the Volcanoes of the Moon, read before the Association of Geologists and Naturalists, in September last,† some suggestions were thrown out with regard to the Origin of Continents, drawn from the condition of a cooling globe. It was observed that the portions of the earth now constituting the great areas of land, were free, or nearly so, from volcanic action, even in the Silurian period: while the oceans appear to have been regions of eruption. Hence it was inferred that contraction must have taken place to the greatest extent over the parts now oceanic, just as any cooling sphere becomes depressed on the side which cools last. This was shewn to correspond with the actual history of our globe, inasmuch as an increasing depth in the ocean cavity would necessarily leave more and more land above water in successive epochs, as accords with observations. It was observed that the hypothesis was *farther* borne out by facts: for while it appears that the land has, on the whole, been increasing in extent, even through the tertiary era and subsequent to it, the ocean's bottom has actually subsided several thousand feet within a late period, as shewn by the coral islands scattered over the wide Pacific.*

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* Extracted from the American Journal of Science, vol. iii., second series.

† See Amer. Journal of Science, vol. ii., second series, 352, and also page 10 of this, the 43d volume of the Edinburgh New Philosophical Journal.

By reference, therefore, to the principle of unequal contraction, and to those *subordinate* causes of change of level usually appealed to by Geologists (though treated of commonly as primary in importance), we may obtain a general view of the origin of the earth's features. I propose at this time to offer a few remarks in illustration of this subject, derived from the features of our own continent, reserving a fuller discussion for another occasion.†

The effects of contraction as a geological cause, though long admitted, have been first brought out in their various bearings by M. Constant Prévost, before the Geological Society of France.‡ The facts adduced substantiate his views, though, as we believe with some limitations. They lead us farther to connect the various phenomena, and tell why the ocean and land have their present bounds.

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* If we consider that *two hundred islands* have subsided in the Pacific, which had there been no corals, would have disappeared without a record, we perceive that the comparative absence of islands from the Atlantic, whose waters are, to a large extent, too cold for corals, proves nothing against the hypothesis. On the contrary, so large a bare surface of waters is probable evidence of the disappearance of some points of land by submergence. All existing Atlantic islands are of igneous origin except the Falklands, to the east of Tierra del Fuego.

† We may here mention one or two facts in corroboration of the general theory, that the more igneous portions of the globe have contracted most and thereby became submerged. For example we find the continent of America reduced to a narrow strip of land, just where the great American tract is crossed from east to west by a region of igneous action, not yet entirely extinct; that is, about the West Indies and the adjoining isthmus. This region became thus depressed and submerged, in consequence of greater contraction below; and hence North and South America are nearly disjoined by a broad arm of the ocean. This single instance is the only one, through the continent of America, of volcanic eruptions east of the great western chain of mountains.

Again, the East Indies, another region of perpetual fires, in the earth's history, constitute a cluster of islands separating from Asia the large non-volcanic New Holland, properly a part of a south-eastern extension of the continent. Moreover, we may account for the fact that this Archipelago has not farther subsided, so as to become a deep ocean with few islands, on the ground that extensive areas of land, without fires, exist in the midst of the group, Borneo being one example, equalling in extent half the United States, east of the Mississippi. The Indian Ocean, at the same time, bears evidence, in its coral islands of a much more extensive subsidence.

‡ See American Journal of Science, vol. ii., ser. ii., p. 355. While thus men-

In order to understand the bearing of the facts, we should bring to mind the effects of contraction. The more prominent are as follows :—

1. Depressions, provided the contraction be unequal in different parts.

2. Apparent elevations, as a consequence of the depressions; that is, elevations as compared with the lowest level, or with a body of water occupying the depressions.

3. Fissures.

4. Ejection of igneous matter, at times, through fissures.

5. Upheaval along a line of fissure, the surface adjoining being more or less raised.

6. Upliftings and foldings from lateral pressure.—An arc of the exterior surface being greater than any corresponding arc below the surface, a depression of the hardened exterior, produced by the cooling beneath, would in some instances cause lateral displacements.

7. An *unequal rate* of subsidence over given areas in different periods.—Contraction tends to occasion a strain upon the cooled and unyielding exterior, accompanied generally by a consequent diminished rate of subsidence, or a cessation of it. This strain increases till it results in fractures; and following this crisis, subsidence would for a while be more rapid in rate. The strain, or state of tension, might also occasion elevations in some places, within or without

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tioning the name of M. C. Prévost, we should remember that the theory of contraction, as a cause of the earth's features, dates as far back as Leibnitz, many of whose speculations in science are proving to be as well founded as the rigid results of his mathematics. And among the geologists of the present day, De la Beche especially has insisted upon this agency as the general cause of the unevenness of the earth's surface, though he stops by stating some of the grand results, without allowing them their full influence as laid down by Prévost. Mr Lyell, in his *Travels in North America*, has made a partial application of the principle to the Appalachians.

The writer does not claim to have presented any new principle, except it may be the special cause assigned for the oceanic depressions; and whether this holds true, remains for the future to determine.

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l be prolonged intermissions in the subsi-
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calculations of William Hopkins, Esq.* But
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orders of such a region; and they would con-
ive fissures ranging along the area, and an
lling of the surface, or else a rising of the
ls by lateral pressure.†

Phil. Soc., vii., 22.

to the *folding* of strata by lateral pressure, the theory was
Sir James Hall, (Trans. Roy. Soc. Edinb., vii., 85,) and the
e, coupled with the elevation of the land, was suggested by
source of the pressure in the instances he mentions. Scrope,
subject, says, in his work on volcanoes, published in 1825,
to conclude that in most instances, the raised strata, par-
ich were only partially indurated, have been contorted and
so as to give the appearance of frequent alterna-
ta to what is in reality but the replication of
201. De la Beche applies the theory to the
earches, 129,) the possibility of which applica-
Hall.
ed Sir James Hall in considering that besid

The effects of lateral pressure might in many parts be local or of very limited extent. A contracting area might be made up of several separate areas of contraction not acting together upon any particular line. Even supposing a whole quarter of our globe to exert laterally all the force possible, by a uniform contraction continued till the surface was depressed eight miles in depth, the whole effect would be equivalent to a lateral dislocation of only twelve miles. And in this calculation, we make no allowance for upliftings over the contracting area, which would diminish the action; nor for a diminution of breadth in the surface of the area, which diminution must be going on if the surface is losing heat. In the remarks which follow relating to this point, America, therefore, is not instanced as an example of what *must everywhere* have happened, but of what *has here* happened.

The foregoing are the obvious effects of contraction. A *Prince Rupert's drop* (a drop of unannealed glass) may be referred to for farther illustration. The exterior, owing to its cooling first, is under strong tension, and each particle (or section) in the surface, presses laterally upon its neighbour like a stone of an arch upon the one adjoining; and hence the effect of a simple scratch in causing it to break to pieces, explosively. The earth, had it cooled uniformly over the whole exterior (and were it made of a uniform homogeneous material), would have been in the same circumstances, the whole crust being under immense tension, yet everywhere balanced, and therefore not apparent; but cooling unequally, the same actual amount of force has been exerted, yet at different periods, producing, in different parts and in different periods, fractures, depressions, and upliftings.

We comprehend the effects described more clearly if we remember, as we ought, the common statement, that the highest mountains of the earth are about equal in compara-

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the lateral pressure, pressure *from above* is essential to this result. But since the soft strata are *inelastic*, and moreover, in themselves *are of vast weight*, we may conclude that there is sufficient vertical pressure independent of any foreign source. A small hand model appears to be as suggestive of error in this case, as a child's model of a bridge to the inexperienced bridge builder.

tive altitude to the thickness of the cracked varnish on a twelve inch globe.

We remark, again, that we exclude none of those causes of elevation usually recognised, which facts shew to have been in operation, though allowing them only a subordinate place.

From these explanations we proceed to the application of them.

If the reader will place before him a good map of North America, he will perceive at once the effects which have been alluded to exhibited on a grand scale, on both sides of the continent. On the *Atlantic* side, the Appalachians, from Maine to Georgia, consist of rock strata, which have been variously folded up into ridges, as has been made out with great beauty and fulness by Professors W. B. and H. D. Rogers.* These folds are in several series, but are nearly uniform or parallel in position. As should be expected from the nature of the cause, the plications are more frequent and abrupt on the side of the chain nearest the ocean, and gradually die out westward just beyond the limits of the Appalachians. As another result of proximity to the contracting area, the rocks on the eastern side have been most altered by fire. To so great a degree has the heat operated, (which escaped by the opened cavities and fissures, and was distributed laterally by the aid of the contained and incumbent waters,) that it is difficult in New England to distinguish the true igneous rocks from those that are metamorphic.

On the *Pacific* side of the continent, we observe the Rocky Mountain range rising with a gentle swell from the coast. From the mouth of the Kansas to the top, and on the opposite or western side, the average slope is hardly twelve feet to the mile.† The summit is about eight thousand feet high.

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* Trans. of the Assoc. of Amer. Geol. and Nat., 1840, 1842, p. 474, and Amer. Jour. of Science, xliii., 177; xliv., 359.

† See the section of the region between the mouth of the Kansas and Fort Vancouver, by Captain Fremont, in the Report of the Exploring Expedition to the Rocky Mountains in 1842, and to Oregon and North California in 1843, 1844. Printed by order of the Senate of the United States, Washington, 1845.

But there are ridges which add five or six thousand feet to the chain: these form a crest to parts of the range, but are not properly the range itself, though often so recognised. The Rocky Mountains appear, then, to be another effect of contraction, viz. a gradual swelling of the surface, accompanied by fissures and dislocations over its area. These dislocations are very marked in the sandstone, just east of the summit. Thus each great oceanic depression, the Atlantic and Pacific, has its border range of heights thrown up by the very contraction which occasioned the depression; and between lies a vast plain, scarcely affected at all by these changes, the great central area of the continent. This view is farther sustained by finding that the effects of fire are most apparent on the ocean side of the mountains, precisely as about the Appalachians, yet to a more remarkable extent.* Indeed, there are no remains of volcanoes, or their ejections, to the east of the summit; while to the west, the country of Oregon is in many parts buried beneath basaltic or other volcanic rocks, and several existing volcanic cones have been described. Still farther, we observe a second, a third, and even a fourth, parallel range of heights from the summit of the mountains to the coast; and the third (the Cascade range) rivals the Rocky Mountains in the height of some of its snowy peaks. Vast fissures were opened to the fires below, as these ranges indicate, and some of the vents have not

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* The same is the general character of the Andes. In an account of the geology of Chile, M. I. Domeyko says, speaking of the Andes in the latitude of Copiapo, " En regardant du côté de l'Ouest, on voit un bouleversement complet dans le terrain soulevé : des failles et déchirements, des escarpments à pic, des stratifications contourneés et interrompues. En portant ensuite la vue du côté de l'est, on voit des pentes douces, des bancs de rochers presque horizontaux et rarement interrompus.

" Tout announce que le principal mouvement qui survint à l'époque de la formation des Andes arriva *du coté de l'Ouest*, c'est-à-dire du côté où une ligne d'escarpments qui marquent le rivage actuel de l'Ocean depuis le Cap Horn jusqu' aux Montagnes Rocheuses, continue à se soulever d'une manière lente et à peine perceptible, au mugissement de bruits souterrains et sous l'influence des tremblements de lerre répétés."—*Annales des Mines, iv. ser.*, ix., 413, *2nd. liv.* 1846,

yet ceased action.* Here, then, are the natural effects of proximity to a region of contraction—the Pacific—in which the remains of igneous action everywhere abound.

It has been well established that the Appalachian folds or plications were made since the coal period, for the coal beds are enclosed in the folds; † and the rising of the Rocky chain was also subsequent to that era. The effect of contraction in producing these elevations, was therefore comparatively little felt in the very earliest ages, when the surface of the depressed (or igneous) portion was itself somewhat yielding, but subsequently, when it had become stiffened to a considerable depth by cooling. There appears hence to be a perfect harmony between the results and the causes adduced.

If these conclusions are correct, we must give up the popular idea (at least as a general theory) of the elevation of mountains by a force below causing at the time an irruption of igneous matter; for the irruption is in general an effect of a very different action, as has been urged by Prévost. This may be as true of the Urals, as of the Rocky Mountains and Andes.

Even the trap-dykes of New England and New Jersey, whose general course corresponds with that of the Appalachians, may be a result of the contraction in progress subsequent to the coal era. The dip of the new red sandstone accompanying them is probably another effect. The Ozark Mountains, forming a line parallel with the Appalachians, beyond the Mississippi, may be referred to the same system of changes.

The economical advantages belonging to the features of North America that have thus originated, are most remarkable, and this view of their origin gives them increased interest. The Silurian rocks indicate that before the coal period the region was comparatively level, and lay mostly beneath the sea. As it emerged it was still dripping with water, so that, under a climate peculiarly genial, coal vegetation might have grown luxuriantly. But had it continued thus flat to a later period, it would have had but small streams, and probably, for want of a mountain barrier to intercept the *drying* Pacific winds, the desert regions of the west would have traversed the land, as Sahara has spread over Africa. As if to prevent these results, and give a vastness scarcely equalled to its resources, the land was raised into mountains on either coast, those of the west, where the barrier was most needed, ascending even to the regions of perpetual snows. The whole interior is now inclosed by the Rocky Mountains on the one side and the Appalachians on the other, and a thousand streams are set in motion over the wide land from either bound, all to contribute to a common trunk, the great highway of the country. Thus the largest possible extent of intercommunicating inland waters has been secured; and for the same reason a great part have been made to flow so nearly on a plain as to afford navigation almost from one end of the territory to the other, and extend their fertilizing influence over the whole surface. A similar result has been produced on the narrow ocean side of the main chains by the succession of parallel coast ranges; for the waters have been compelled to flow far north and south between these ranges, and fertilize an extended country before the sea was reached. Thus the noble Columbia, with its wide spread tributaries, was made for Oregon; and in the same manner were formed the Willammet, the Sacramento, and the Joachim, which run in long courses between the Cascade and Coast ranges of heights. Thus on the Atlantic side, we have the Shenandoah and other head waters to the Potomac, and at the north, a Hudson, Conecticut and Merrimack flowing in parallel lines.

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* Granites may have been the earlier products; but the existing volcanic mountains have basalts and trachytes for their surface rocks.

† W. B. and H. D. Rogers, Trans. Assoc. Amer. Geol. and Nat., 1840-1842, p. 522.

Note.—In connection with this article, it should have been earlier mentioned that the theory of "secular refrigeration" has been presented with much force, in many points of view, by W. W. Mather, in the American Journal of Science, vol. xlix., p. 284 (1845), and the foldings of the Appalachians are attributed by him to this cause.

On Sulphato-Chloride of Copper, a new Mineral. By ARTHUR CONNELL, Esq., Professor of Chemistry in the University of St Andrews.* Communicated by the Author.

Some minerals were lately put into my hands by Mr Brooke for chemical examination. Amongst these was one which, on examination, I found to be a new combination, viz., a sulphato-chloride of copper.

This mineral occurs in small but very beautiful fibrous crystals of a fine blue colour, which is pale when the fibres are delicate, but much deeper when they become somewhat thicker. Their form, Mr Brooke informs me, is a hexagonal prism with the edges replaced, thus belonging to the rhombohedral system. They possess considerable translucency, and have a vitreous lustre. The crystals are too small, and the quantity of them at my command too inconsiderable, to enable me to state their specific gravity, hardness, or fracture. Their locality is Cornwall; but I do not know what part of that county. Mr Brooke is aware of the existence of only a very few specimens of the mineral. One is in the British Museum.

Like Atacamite, this mineral colours the blow-pipe flame as well as the simple flame of a candle, a fine greenish blue, indicating the presence of chloride of copper. Reduced to powder, and mixed, in sufficient quantity, with charcoal powder, and then heated in the close tube, it gives decided, although not strongly marked, indications of the presence of sulphuric acid by the smell, and partial bleaching of brazil-wood paper, the remainder of the paper being reddened, doubtless by muriatic vapours. Alone, in the close tube, it yields a little water, and other appearances resembling those afforded by Atacamite. Heated alone on charcoal before the blow-pipe, it decrepitates strongly, but when previously deprived of the greater part of its water by gentle heat, and then powdered, and moistened, and heated on charcoal, it gives no traces of arsenic, although arseniate of copper [illegible] associated with it in the specimens. The residue is a dark reddish slag or globule.

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* Read to the Chemical Section of the British Association at Oxford, in June 1847.

The crystals are not soluble in boiling-water, but dissolves entirely, and pretty readily, in nitric or muriatic acid, especially by the aid of gentle heat. The solutions have the colour belonging to copper solutions, and in the act of dissolving, a very few bubbles of gas may be observed to arise, indicating probably the presence of a minute quantity of carbonate. The solutions yield, with barytic salts, a white precipitate insoluble in acids; and the nitric solution gives, with nitrate of silver, a white and curdy precipitate, insoluble in acids or water, but dissolved by ammonia, Ammonia, in excess, added to the original solution, gives the fine deep blue of copper.

These appearances, in conjunction with the blow-pipe reactions, are sufficient to shew that the constituents of the mineral are sulphuric acid, chlorine, copper, and a little water. I had not enough of material to determine the proportions of these constituents, but there can be no doubt that the mineral consists essentially of sulphate and chloride of copper, with a little water. Whether the copper salts are neutral or basic, it is impossible to say. The chloride is apparently the more abundant of the two salts. As above stated, there seems also to be a trace of carbonate.

On the Decomposition of Water by Platinum and the Black Oxide of Iron at a white heat, with some observations on the theory of Mr Grove's Experiments. By GEORGE WILSON, M.D., Lecturer on Chemistry in Edinburgh. Communicated by the Royal Scottish Society of Arts.*

The remarkable discovery recently made public by Mr Grove, that water in certain circumstances, when raised to a white heat, is resolved into its constituent gases, has naturally excited much attention. It furnished the unexpected confirmation of the truth of an opinion expressed by James Watt so far back as 1783, that if steam could be made red hot [white hot] so that all its latent heat should be converted into sensible heat, either the steam would be converted into permanent air, or some other change would take place in its constitution.*

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* Read before the Royal Scottish Society of Arts, 10th May 1847. This paper was previously communicated to the Chemical Society of London, in whose Transactions for 1847 it was published. G. WILSON.

In the greater number of Mr Grove's experiments, water was raised in temperature through the medium of platinum; and it became a question accordingly, as Sir John Herschel and my friend Dr Lyon Playfair suggested, how far the decomposition of water observed was owing to the mere heat of the metal, how far to the peculiar surface-influence, or so-called catalytic force, which has been so long recognised as possessed by platinum and the other noble metals. Dr Playfair also referred to the fact, "that many bodies at high temperatures exhibited a great affinity for oxygen, which they did not possess at lower temperatures; as, for instance, silver, gold, and even platinum itself, which metals absorb oxygen when intensely heated, and give it out again on cooling. If the experiments had been tried in tubes of quartz or silica, they would not have been open to the objection which the use of so peculiar a metal as platinum appeared to involve."†

There was indeed one form of Mr Grove's experiment not liable to the exception urged against those where platinum was used. He found it quite possible to decompose steam by sending Leyden-jar discharges through it, and refers the decomposition solely to the heat evolved by the electric spark. The same view has been suggested as not improbable by Faraday, in relation to the decomposition of water in the liquid form by electric discharges.‡ With great diffidence, however, I would remark, that the spark decomposition of water cannot be regarded as an *experimentum crucis.* Although the electric spark cannot decompose steam electrolytically, we may not at once infer that it cannot decompose it in another way. I have no wish to assert that it can, but it is possible that it may, and a crucial experiment should be unexceptionable. Again: the spark discharge of a Leyden-jar exerts a great disruptive force, and acts topically with much violence. There is reason moreover to believe that mechanical agitation or disturbance of a chemical compound can in many cases cause the separation of its elements. It may seem an extravagant idea to suppose that oxygen may be torn or detached from hydrogen by the action of a disruptive force on the molecules of water, as if chemical affinity were but a kind of mechanical cohesion, which may be overcome by division. On the other hand, however, it must not be forgotten, that we are now acquainted with a large number of fulminating compounds, which can be decomposed by friction, by a touch, or a stroke. These compounds are all fragile, and water is a very stable combination; but fragility and stability are but terms of degree, in relation to Chemical union: and if it shall appear that a feeble mechanical force can overcome a small intensity of affinity, it will be acknowledged as quite possible that a powerful mechanical agency may overcome a great one. We have no means perhaps of making an unexceptionable experiment as to the decomposing power of mechanical force; for we cannot bring it into play without calling into action other agencies. If we touch, or rub, or strike a fulminate, for example, we cause the evolution of heat, and add its decomposing power to that of the mechanical impulse. It would be a mere *petitio principii*, however, to assume that the heat produced alone effects the decomposition observed. It seems to me, therefore, that the decomposition of steam by the electric spark furnishes a more complex problem for solution than the action of white-hot platinum on the same compound does; and that the experiments made with the metal are more likely to throw light on those tried with the spark, than to be explained by them.

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* Phil. Trans. 1783, p. 416.

† *Athenæum for September* 19, 1846, p. 966.

‡ *Researches in Electricity*, 3d Series, paragraph 337.

Whilst thinking over these difficulties, and the objections to Mr Grove's conclusions suggested by Herschel and Playfair, I had occasion to perform the familiar class-experiment of burning iron-wire in oxygen. I observed with an interest I had not felt previously, although I had carelessly noticed the phenomenon before, that bubbles of apparently permanent gas rose from the globules of white-hot oxide of iron as they fell into the water. It seemed to me possible that this gas might be a mixture of oxygen and hydrogen separated by the influence of the metallic oxide, acting as platinum did in Mr Grove's experiments. It was certain, moreover, that if this should prove to be the case, it would supply a powerful argument in favour of that gentleman's conclusion, which seems, in spite of all the objections noticed, in the highest degree probable, namely, that heat, apart altogether from the medium through which it is applied, can resolve water into its elements.

As the following experiments were made solely with the hope of substantiating Mr Grove's view, which unfortunately, however, they leave exactly as they found it, I trust that gentleman will not consider their publication an interference with his researches. I was led to try them incidentally, and abandoned them as soon as I found I could render Mr Grove no assistance by means of them.

It would be difficult to conceive a more rapid and effectual way of raising a body to a white heat than that afforded by the combustion of iron in oxygen. I took for granted also (as it afterwards appeared, too hastily) that the metal could not but be saturated with oxygen and converted into a definite oxide, which would be chemically indifferent to each of the elements of water, and if it decomposed it at all, would reject both its constituents. The convenient way, moreover, in which the globules of oxide detach themselves and fall into the water, and the rapidity with which the whole process goes on, make it a very easy matter to collect in considerable quantity whatever gases are evolved. A stoppered bottomless jar of the ordinary construction for the iron-wire experiment, and of 291 cubic inches' capacity, was made use of in the following trials. Eighteen experiments were made with it, and from 100 to 110 grains of fused globules were obtained from each combustion. A test-tube, with a funnel fixed into it by a perforated cork, and filled with water, was arranged so as to receive the gas. In some experiments it was placed within the oxygen jar, so that the coil of wire when introduced hung close to it, a piece of tin plate being arranged so as to guide the globules within the edge of the inverted funnel. In the greater number of trials, however, the tube and funnel were placed outside of the vessel containing the oxygen, and an inclined plane of tin plate was so sloped as to carry the globules past the edge of the jar, and within the mouth of the funnel. No difference of result was observed in experiments made in both ways, but the latter arrangement was preferred as more convenient, and as enabling more oxygen to be employed at each trial.

In all the experiments, permanent gas was evolved when the fused globules fell into the water. This statement is to be considered as applying to each combustion considered as a whole; for individual globules were frequently observed to give off no gas at all, or to evolve so very little, that it might be air separating from the water, in which it had previously existed in solution. The quantity of gas obtained at each combustion varied greatly. Sometimes as much as a cubic inch was procured, more frequently only half that quantity, and occasionally less. The globules from thick coils of wire gave off a larger volume of gas than those from thin ones.

Portions of the gas were transferred to a Grove's eudiometer over water, and exposed to a white-hot platinum wire. They did not kindle or detonate, nor were they sensibly diminished in volume Other portions were subjected to electric sparks and discharges in a syphon eudiometer over water, with the same negative results; but when air or oxygen was mingled with the gas, it exploded sharply with heated platinum or the electric spark. When a match was applied to the open end of a tube containing the unmingled gas, it burned rapidly with a pale blue flame, but did not explode. The gas given off during the action of the fused globules on water, was not, then, a mixture of oxygen and hydrogen.

Its freedom from all but a trace of oxygen was ascertained in other ways. To one portion of the gas standing over water nitric oxide was added, but no ruddy fume or yellow coloration shewed itself. When phosphorus was introduced into the gas, in one instance it did not smoke, but in the greater number of cases it fumed for a brief period, and occasioned an amount of contraction barely perceptible. The gas appeared to be nearly pure hydrogen. To ascertain if it certainly were so, a portion of it was carefully dried, by chloride of calcium, and transferred to a eudiometer over warm mercury. Dry oxygen was then added, and the mixture exploded. When the whole had cooled, the walls of the eudiometer appeared dimmed by a very thin layer of moisture, but the quantity of gas operated on, was too small to admit of visible drops being produced. Another portion of the gas was mixed with half its volume of oxygen and fired by the electric spark. The contraction which followed explosion, varied in different experiments, but was frequently such as to leave not more than one-twentieth part of the mixed gases unconsumed. Phosphorus smoked in this residue for a short time, shewing that excess of oxygen had been made use of, and left a minute volume of gas which was not diminished by caustic potash, and must have been nitrogen.

It seemed possible that the trace of carbon present even in malleable iron, might affect the quality of the gas resulting from the action of the globules of oxide on water, and that carburetted hydrogen, carbonic oxide, or carbonic acid might be produced. It seemed desirable to know whether the latter were present or not, as the oxygen might have gone to form them. It was impossible to be certain that carbonic acid was absent, for the gas from the globules being necessarily collected over water, the temperature of which was low, carbonic acid would be retained in solution by that liquid. All that I can say on this point is, that lime-water was not rendered muddy or in the slightest degree opalescent by the gas. It was several times detonated with oxygen over lime-water, but the latter remained quite transparent, so that neither carbonic oxide nor carburetted hydrogen can have been present. In short, the gas evolved from water by the white-hot globules of oxide of iron, was hydrogen mingled with a small quantity of air, previously no doubt in solution in water.

As only the hydrogen, then, of the water decomposed was obtained, it became necessary to account for the absence of the oxygen. I was tempted for a moment to think it possible that the black oxide of iron might have changed into the red oxide of the same metal, by combining with the oxygen not obtained in the elastic form: *ex. gr.* thus $2\ Fe_3\ O_4 + O = 3\ Fe_2\ O_3$.

But the proto-peroxide of iron is known to be a very stable compound, little if it at all prone to become the peroxide; and it seemed more likely that unoxidized iron might be present in the fused globules, which occasioned the evolution of hydrogen when it came in contact with water. To ascertain this point, portions of the globules were dissolved in dilute muriatic and sulphuric acids, and were found in most cases to evolve hydrogen. Some specimens of the globules gave off not a trace of gas when they dissolved, and must have consisted of the definite oxide; a point of interest in connection with the fact already mentioned, that globules were frequently observed to drop into water without any bubbles of gas rising from them.

The volume of hydrogen however given off in some of the trials, when the product of combustion was placed in acid, was very considerable. A graduated gas jar was filled with dilute sulphuric acid, and inverted over a small capsule containing 100 grains of the crushed globules, which was placed in a basin also containing dilute acid. By this arrangement the gas was collected and measured at the same time, without risk of mixing with air, or necessity for watching the process, which is a slow one. 100 grains treated in this way gave off 16 cubic inches of hydrogen, corresponding to 9 grains of iron. The experiment was accidentally stopped at this point whilst the gas was still rising in undiminished quantity.

Metallic iron, then, was certainly present in many of the globules, and of this I had direct ocular demonstration. On crushing some of them in a mortar, they were found to separate into a shell of pulverizable oxide, and a core of iron which formed a nearly spherical pellet. In one case 50 grains of the globules were crushed, the pellets separated, and the residue placed in diluted sulphuric acid. It did not evolve a trace of hydrogen in the course of twenty-four hours. The pellets were then added to the same acid, and gave off 12 cubic inches of gas = 13·6 per cent. of iron in the globules.* The shell of oxide is frequently imperfect or perforated, so that water or any other liquid penetrates to the iron core, and is subject to its influence. When this becomes known, it need not surprise us that most of the globules should rapidly decompose water. After observing this fact, I tried the effect of thick and thin coils of wire, and found that the former invariably gave off the greater volume of gas. When the coil is so thin that the metal is all oxidized, no gas is evolved at all. A thick coil, indeed, furnishes a striking mode of illustrating to a class the principle of Lavoisier's mode of decomposing water, and forms a beautiful addition to the iron-wire experiment.

From these observations then, it would seem that white-hot oxide of iron cannot decompose water in the way white-hot platinum does. But before any conclusion can be drawn from this fact inimical to Mr Grove's views, or favourable to the opinion that a specific property of the platinum has more to do with the decomposition of water than its mere temperature has, we should require to know how far the two white-hot bodies are to be considered as at the same temperature. In Mr Grove's experiments, platinum is raised to as high a heat as it can bear without fusing. It must then be elevated to a temperature much above that necessary to make iron white-hot, or to fuse its oxide, for our forges can melt iron and its oxides, but do not fuse platinum. It may also be remarked, that bright as the light emitted by burning iron is, it falls short in intensity of that given off by platinum on the verge of fusion. It seems accordingly probable, that during the combustion of iron in oxygen, the temperature never rises high enough to confer upon the resulting oxide the power of decomposing water. The question admits of direct decision, by ascertaining whether oxide of iron, heated by the oxyhydrogen blowpipe to as high a temperature as fusing platinum, acquires the power of decomposing water without appropriating to itself either of its elements. But it would have been an interference with Mr Grove's own researches, to have made experiments of this kind, and I have accordingly left the question undecided.

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* In none of the experiments was the thermometer or barometer specially observed, as minute accuracy was not aimed at.

Meanwhile, the experiments I have recorded are of some little interest, as at least shewing that not only a white heat, but a high white heat, is essential to the successful performance of Mr Grove's experiments. Unfortunately, we have not at present any method of measuring high temperatures which admits of ready application or secures great accuracy. "White heat" is in fact a vague expression for a range of temperature; of the extremes in either direction or extent of which we have no very precise knowledge. The power of the eye to measure the relative intensities of the light evolved by white-hot bodies is very limited, and varies greatly in different individuals. But the experiments I have recorded, seem to supply the means of so far defining the white heat requisite for the separation of the elements of water, inasmuch as they shew that it must at least exceed the temperature necessary for the fusion of malleable iron or its black oxide. If, moreover, the decomposing powers of the electric spark be solely referable to its temperature, we seem entitled to conclude, from the experiments I have detailed, that the heat of the smallest spark that can decompose water is at least equivalent to that of fusing platinum. They appear also to warrant another conclusion. It was suggested by Dr Leeson and by Mr Hunt, that the bursting of steam-boilers might occasionally be owing to the metal they consist of becoming white-hot, and decomposing water, as platinum does, with the rejection of both its elements.* This ingenious suggestion seemed to myself, before making experiments with iron, likely to prove just; but as fusing white-hot iron appears unable to decompose water, otherwise than by combining with its oxygen, it is impossible that the walls of a boiler can ever be raised to a temperature sufficiently high to enable them to separate the elements of water in the way platinum does.

I may now be permitted to make some comments on the rationale of the results obtained by Mr Grove. That gentleman, if I understand him aright, considers the decomposition of water by white-hot platinum not only, as assuredly it is, a remarkable and unexpected result, but as evidencing on the part of heat a power to produce op-

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* Athenæum, 19th Sept., p. 966.

› or dissimilar chemical effects in the same circumstances. He ›orted in the Athenæum (Sept. 19, 1846, p. 966,) to have :ounced his discovery that all the processess by which water may rmed are capable of decomposing water," (p. 966). If, by tatement, be simply meant, that heat combines oxygen and gen into water, and decomposes water into these gases, it will nitted to be a just conclusion; but it may be questioned, I whether Mr Grove's experiments add anything to our know- of the power of heat to effect chemical changes, except in so they supply an additional very remarkable example of its two- nalytical and synthetical agency, which has been so long recog- Hydrogen, which as a gas, is probably the vapour of a very e metal, may be compared with mercury, also a volatile sub- . If mercury and oxygen be heated together to the tempera- f 662° F., they combine and form the red oxide of the metal. › resulting oxide be raised to a low red heat, it is decomposed ıercury and oxygen. In like manner, if hydrogen and oxy- ɜ raised together to the temperature of 660° F.,* they unite rm water. If the resulting water be raised to a white heat, ɜsolved into hydrogen and oxygen. Both metals (?) present ne phenomena. At one temperature (nearly the same in both combination with oxygen occurs; at a higher temperature, position of the oxide happens. Many other examples might en in illustration of the same fact. Such cases, however, do ɜm to warrant a conclusion as to heat exhibiting anything like rity of force, by which I understand the manifestation in op- directions of opposite powers of equal intensity. At all events, opposite effects of different *intensities* of the same agent be con- d equivalent to a polarity of action, it is difficult to see what nay not be called a polar one. The decomposing and combin- ɔwer of heat of different intensities, seems exactly comparable opposite effects of different intensities of mechanical impulse. o pieces of smooth glass are laid together and struck gently ɜpressed slightly, they unite or cohere. If the united pieces ereafter exposed to a sharp blow, or to great compression, the is dissolved, or they are shattered to fragments. Here the force effects mechanical synthesis and mechanical analysis. ı these contrasted actions, as seems to be the case also in Mr ›'s experiments, the results are occasioned by a difference in ɜ of intensity of the same power, not as in the opposite effects polarizing force like electricity, by a difference in the kind of , which appears whatever be its intensity.

ere is one form, indeed, of Mr Grove's experiment which at ight does not appear to admit of the explanation proposed ference to the other trials—I allude to the decomposition of steam by the electric spark, which is well known to have t power of combining hydrogen and oxygen into water. A si experiment was made in perhaps a still more instructive form the latter part of last century by Beccaria,* Pearson, and V Troostwyk, and more recently by Wollaston,† in his well-kno decomposition of water by guarded poles. In certain of these tri it was found that Leyden jar discharges sent through water, d composed it till the accumulation of permanent gas left the wi bare; after which, the first spark that passed recombined t gases into water, which again covered the wire, when decompo tion could anew be obtained. Here, to appearance, the same ag acting with the same intensity, alternately decomposed and reco posed water. For argument's sake, let it be acknowledged tha the heat alone of the spark was the cause of the chemical chang Nevertheless, it may be questioned, whether it acted with equal i tensity in both cases. The electric spark must be conceived, acco ing to the results already given, to be at first at a high white he and whilst retaining this temperature, we may believe it to posses power of disuniting the elements of water, and of preventing th union. But, as soon as the spark falls to the temperature of 66 F., it loses its power of decomposing water, and, on the other han acquires a power of uniting hydrogen and oxygen. Although, ther fore, the spark is always *furnished* of the same intensity, its acti may change, and even be reversed, as its intensity diminishes. Mor over, even when the spark is white-hot, it is only the amount o matter directly in its track that will be raised to a white heat. Contiguous portions will have their temperature much lower, so that in the case of hydrogen and oxygen, at some little distance from the route of the spark, the temperature will be 660° F., and there combination will begin, and ultimately extend, through the whole mass of gas.

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* *Graham's Elements*, 1st edit., p. 259.

In like manner, when a platinum wire is made white-hot in a mixture of hydrogen and oxygen, it causes their combination. Here we may suppose that union occurs as soon as the temperature of the metal rises to 660° F., and before it acquires a white heat. Or if we were to arrange matters so that the wire should be made white-hot in a vacuum, and hydrogen and oxygen afterwards admitted to it, still union of the gases should happen; for although the wire might prevent combination immediately around itself, at no great distance where the temperature was below 700° F. it would compel union. In all such experiments the combining effect of heat will be much more manifest than its decomposing power; not that perhaps the former is in reality greater than the latter, but because flame is propagated through a mixture of hydrogen and oxygen by a series of ombustions. The hot wire or the electric spark kindles only the ortions of gas immediately adjacent to it, but the combustion of hose sets fire to the molecules contiguous to them, and these in their urn to their neighbours, till all are made to burn. Thus the flame ravels after the original cause of combustion has ceased to operate irectly, and the momentary action of a small spark, or the transient eat of a red-hot capillary wire may suffice to fire an infinitely large ass of hydrogen and oxygen. There is no provision for a similar ropagation of decomposition through water or steam when either is ade white-hot; the absolute amount accordingly of disunion of the lements of water occasioned is very small.

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* Lettere dell' Elettricismo, quoted in Lardner's Electricity, vol. i., p. 78.
† Faraday's Electrical Researches, series 3, paragraph 328.

If allowance, however, be made for the apparent difference in ex-ent of effect which heat shews in uniting and in disuniting the ele-ents of water, the phenomena otherwise seem referable solely to he intensity of the temperature to which hydrogen and oxygen are xposed. The opposite processes might go on simultaneously, union r disunion being determined simply by the different temperatures to which different portions of the gases were raised. At least it seems not improbable that if a mixture of steam and of hydrogen and oxygen were exposed to electric discharge, decomposition of the steam and combination of the hydrogen and oxygen might be effected by the same spark, provided the volume of steam were not large. In the track of the spark, decomposition would occur, so long as a white heat prevailed. When the temperature fell, combination would happen where the spark had passed, if it had not already commenced in the neighbourhood of its direct route. Similar remarks apply, *mutatis mutandis*, to the action of a hot platinum wire on a mixture of steam with oxygen and hydrogen.

It may be objected to this view, that Mr Grove decomposes steam in his eudiometer, and obtains a permanent bubble of gas, consisting of hydrogen and oxygen. The bubble however obtained in this way is very small, and could not probably be greatly increased. Mr Grove has not mentioned how large a volume of hydrogen and oxygen he could obtain in the same eudiometer, by alternately boiling the water till the steam produced caused the liquid to fall below the wire, and allowing the steam to condense till the water rose above the metal. But I venture to say that no large volume of permanent gas could be procured by this process if the same eudiometer were employed many times successively. The combining action of the wire might not take effect on the hydrogen and oxygen when their quantity was small, and they were diluted through a large volume of steam, for in virtue of the law of diffusion, the molecules of hydrogen and oxygen would be separated from each other by molecules of water vapour; but when the latter diminished in bulk, it seems impossible to doubt that kindling of the gases would occur.

Mr Grove's experiments then do not appear to prove that heat of the same intensity is able, in the same circumstances, to form water and to decompose it. When therefore it is stated that water can be produced by the processes that disunite its elements, the word "process" can only be understood to signify that the general arrangement in both cases is the same, not that the intensity of the agent called into play, or its mode of action, is identical. If this could be affirmed, we should be able to announce as a general proposition, that manifestations of the same force, absolutely identical as to quality, quantity and intensity, can produce totally opposite results, which would be tantamount to affirming that unlike effects may flow from the same cause, without any alteration in the qualities or conditions of the latter.

The last observation I would make, refers to the curious fact noticed by Mr Grove, namely, that when a platinum wire is heated white-hot in steam, "in a few seconds a small bubble of gas is formed; but if the action be continued for a week, it does not increase in quantity." *

Are we to suppose that the wire is at the same time decomposing water around itself, and producing water at a little distance, undoing in one place what it effects in another, so that no permanent accumulation of gas is allowed to take place? This is possible, but I think not likely. The observation made by Mr Grove seems sufficiently explicable, on the supposition that as soon as the wire is completely enveloped in steam, the thermo-circulatory currents which the high temperature occasions in the vapour, prevent it from remaining long enough in contact with the wire to become heated white-hot. The steam probably circulates endlessly around the wire without a trace of decomposition occurring in it. It seems not unlikely indeed that in Mr Grove's experiments with his eudiometer it was not steam that yielded the hydrogen and oxygen obtained, but the last film of water below the wire, which could not escape from the metal, but tended rather, in consequence of its expansion, to rise towards it, and was thus compelled to acquire a white heat, and to break up into its elements. If this view be correct, an arrangement where a white-hot wire or sheet of platinum foil was kept grazing the surface of water, might be found to effect a continuous decomposition of the liquid in question.

It is no objection to this view that an electric spark decomposes steam readily, for the duration of the spark is so short, that there is no time for the production of thermo-currents, nor any possibility of the steam escaping from the powerful topical action of the discharge. The spark may be compared to fulminating silver, whose action is instantaneous and violent, but quite local,—the heated platinum to gunpowder, the effect of which is cumulative and more general.—*Transactions of the Chemical Society for* 1847.

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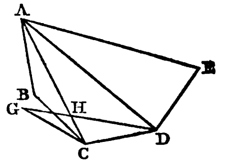
* Athenæum, 19th Sept., 1846, p. 966.

On the Theory of Parallel Lines. By HENRY MEIKLE, Esq. Communicated by the Author.

In my former article on the same subject, in this Journal for April 1844, it was hinted that some doubt might be supposed to attach to a certain part of the proof; but a hope was likewise expressed that my second proposition would lead to something better. Accordingly, by means of it, I have been so fortunate as to obtain the following legitimate substitute for the part in question; and here, as formerly, brevity has been studied, especially in the more obvious details.

Prop. The angles of a triangle cannot be less than 180°.

It was proved, as a case of the second proposition, that if any one triangle had the sum of its angles less than 180°, so would every triangle, and the defect from 180° would always be proportional to the area. Hence, proceeding on the same hypothesis, a very large triangle should have the sum of its angles very minute, and the two at the base still less. Now, it is shewn under the first proposition, that on a base equal to any side or base of any given triangle, there may always be as great an isosceles triangle; but the smaller an isosceles triangle is on an equal base, the smaller evidently will its angles be at that base.



Let ABC be a triangle in which angle B is large, say = 150°, and AB = BC = the greatest side of the foresaid large triangle; and let BC, CD, &c., be an indefinite series of equal bases in as many triangles whose common vertex is A, and in which the angles ABC, BCD, &c., continually decrease, such that those triangles shall gradually approximate to being isosceles, and yet not more than one of them could be perfectly isosceles, otherwise some of the decreasing angles ABC, BCD, &c., would not only be equal, but have that equality beginning with an increase; thus two angles at the bases of the isosceles triangles would exceed the sum of one and the angle just before the first isosceles triangle. Nor could the ***radiants,*** or lines drawn from A, ever begin to decrease without continuing to do so; because otherwise, an increase behoved to occur in the angles ABC, BCD, &c., where the radiants were shortest.

Construct a triangle GCD every way equal to ABC; then since angle DCG exceeds BCD, the equal lines AC, DG, cut each other in H, and unequally too; thus DH exceeds CH, because opposite a greater angle, and so the remainder GH is less than AH. Hence, the triangle AHD exceeds GHC, and, therefore, the triangle ACD exceeds GCD or ABC. Again, because the angles BCD, CDA, respectively exceed CDE, and CDG, or BCA, the angle ADE is less than ACD; and, consequently, it may be shewn in the same way that the successive areas continually increase, the length of the radiants making the triangles great when they approximate to being isosceles.

The value of an angle in the series ABC, BCD, &c., is at first 150°, but on approximating to two angles in a great or isosceles triangle, it will, as was noticed above, be less than the minute sum of angles in a large triangle. Now between 150° and this small value, there may obviously be an angle of every different intermediate value, and each placed in the order of its magnitude. If, then, a small rate of decrease in those angles would cause the radiants to stretch out to an unlimited distance (for since the contrary has neither been proved, nor ought to be assumed, this case must be provided for), and since it is quite evident that a great decrease would ultimately make the radiants also decrease in length, therefore these two results would be so different as to be running into directly opposite extremes; and so there must still be some intermediate rate which would neither cause the radiants to stretch out indefinitely nor yet to decrease. Hence, about A as a centre, and with a radius greater than any of the radiants, a circle could be described which would include the whole series of triangles, and consequently insure their going round the centre A; otherwise, an infinite number of their increasing areas would not nearly cover the finite area *of the circle.*

Since the series of equal lines AB, BC, &c., can thus go more than quite round A, the continual decrease of their angles ABC, BCD, &c., must cause such of these lines as form the smaller of the angles to be coiled inside of those which form the greater; and yet the increase in the length of the radiants just insures the very reverse, or that the smaller angles shall be outermost, which is absurd, but being a legitimate deduction from the supposition that the angles of a triangle could be less than 180°, it therefore follows that they cannot be less.

Now this is what every previous writer on the subject, so far as known to me, has failed to prove; but the proof now given might be greatly varied in its form. My former paper is liable to no objection, so far as regards proving that the angles of a triangle can never exceed 180°. But for that purpose, a much simpler mode of indefinitely increasing the area, is continually to double the sides of an equilateral triangle, which at each time should more than quadruple the area; for if the angles were greater with greater sides, four copies of any equilateral triangle could always be placed within another having sides of double the length.

The principles which I have employed here and formerly are no other than Euclid has used in his Elements, although by slightly introducing some of them at an earlier stage of of geometry than he has done, I have deviated a little from his arrangement. Now if by so doing, I have been enabled to succeed where he had failed, this surely suggests that his arrangement should be altered, and in particular, that his fifth book, which has no dependence on the four before it, should be placed first of all. There is besides something unnatural in having such a very different subject as the doctrine of proportion in the heart of the elements of geometry. Proportion, however useful and necessary as an auxiliary, can, with no more propriety than any other part of algebra or arithmetic, be reckoned geometry.

Errata in former Paper, Vol. XXXVI.

Page 312, the letter P in fig. 1 is misplaced, it should be at the intersection of the lines IL and CN.

Page 316, *lines 33 and 35, for DNQY read* DNGY.

Remarks on some Corals obtained from great depths in the Antarctic Ocean, in a Letter from CHARLES STOKES, Esq., F.R.S., F.G.S., &c., to Captain Sir JAMES C. ROSS, R.N.

"VERULAM BUILDINGS, 23d Feb. 1843.

"MY DEAR SIR JAMES,—I have found much interest in the examination of the specimens you have sent me, which were obtained by soundings, and the use of the dredge at great depths; and have to apologise both for delaying so long to send you some observations upon them, and for sending them now in so imperfect a state.

"The fragments obtained by soundings from 400 fathoms, 11th August 1841, lat. 33°, 31′ S., long. 107° 40′ E., consist of pieces of shells and small corals, none of which appear to have been brought up in a living state, with small angular pebbles, very little rounded by attrition. Among them I find two joints of stems of a small fossil Pentacrinite.

"Among the small sand taken up by the soundings in 400 fathoms, August 1841, Professor Edward Forbes finds portions of spines of Echinus, and of spines of Cidaris.

"Of shells, a small broken Cerithium, mouth of a Resida, a Pteropod allied to Peracle, and a fragment of Cleodora.

"Of Annelides, Spirorbis on the stones.

"Many foramenifera, among which are Textularia, Nodosariæ, and several of orbicular and renoidal forms in abundance.

"The corals brought up by the dredge from 270 fathoms, 19th Jan. 1841, lat. 72° 31′ S., long. 173°, 39′ E., consist of three species of Lepralia: Retepora cellulosa, a small piece in a perfectly fresh and living state; a Retepora or Hornera, much resembling the *Hornera frondiculata* of Lamouroux, in similar fresh condition. The polype cells are salient only at the extremities of the branches. The cells appear also on all sides of the branches, which must distinguish it from the species mentioned. The absence of the openings of polype cells from the exterior side of the branches, is made a generic character of Hornera by Lamouroux, but probably is only of value to distinguish species.

"I name this *Hornera lateralis.* The genus *Hornera* was established by Lamouroux for a division of *Retepora*, of which *R. frondiculata* is the type; but it was not well defined by him. The generic character, as given by Milne-Edwards, is, 'Polyparium branched, composed of tubular cells united together throughout nearly their whole length, with terminal circular openings. The cells all opening on one surface.'

"The cells of the present species are not so decidedly elongated as those of *H. frondiculata*, and it may be said to occupy an intermediate place between *Hornera* and *Retepora*; but this is one of the ordinary difficulties in arranging species in genera. An instance of such difficulty is seen in the species *R. versipalma*, which Lamouroux says (*Expos. Method*, p. 41), 'Est tres voisin des Hornères,' which De Blainville places in the genus *Hornera*, and of which Milne-Edwards says (*Anim. sans Vertebrès*, second edition, vol. ii., p. 279), 'Nous sommes porté à croire que ce Polype ne doit pas être rangé dans le genre Hornère, ainsi que le veut M. de Blainville; il nous parait se rapprocher davantage des vrais Retepores.'

"The present species differs from all that are described in the opening of the cell, which, as is shewn in fig. 3,* is placed laterally immediately below the pointed summit of the cell. It is only at the unbroken ends of the branches that this can be seen, as the ends of the cells seem soon to wear away, and on the general surface of the branches only circular openings are left, which are hardly at all prominent.

"In examining into the relations of this species, we have another proof of the necessity of good figures to determine species. The description given of *R. versipalma* is as follows:—'R. nana, ramosissima; ramis ramulosa palmatis; palmis brevibus variè versis; interna superficie poris prominalis scabra; externa sublævigata.'

"This description might do for the present species, if taken from a specimen of which the summits of the branches were not quite perfect; but as the lateral position of the openings of the terminal cells is not mentioned, there is good reason to believe that the present is a different species. It is to call attention to this point, that the name '*lateralis*' is given to this species, though it is at the risk of finding this character common to others, when specimens are found in a perfect state. It is to be observed, also, that the cells of *H. frondiculata* have not, strictly speaking, circular openings, although the tube of the cell is cylindrical. The end of the tube of the cell is, in the perfect specimens, much higher on one side than the rest, so that the opening becomes elliptic by being oblique to the line of the elongated tubular cell.

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* Plates of the beautiful drawings will be found in the "Zoology of the Voyage," now being published under the superintendence of Dr Richardson and J. E. Gray, Esq.

" A new species of Primnoa, which I name *P. Rossii.*

" A Melitœa, *nov. sp.—M. Australis.*

" A Madrepora ? *nov. sp.—M. fissurata.*

" This is hardly to be considered a true Madrepore, but I am unwilling to make a genus for it.

" I send herewith drawings of the three corals last mentioned, which are perhaps the most interesting of the whole. These drawings are carefully and correctly made; and I will say little of the description of them, as the figures will be more useful than words. Although we have long known that a Primnoa from Norway (of which I will speak presently), is found at a great depth, and some other corals have been taken at from 70 to 100 fathoms; yet it is rare, as far as our present knowledge instructs us, to find any corals, except perhaps some of the Celleporæ at great depths; and I am not aware of any previous instance of a Melitœa or a Madrepora at all resembling those here represented having been found except at a small depth, and in a warm climate; from which I had concluded, that they required more of the solar light and warmth than they could obtain at the depth from which you took those specimens. Your Primnoa is, however, to me the most interesting among them. The genus Primnoa was first established by Lamouroux for the single species previously called *Gorgonia lepadifera*, which is that found in Norway above alluded to. Ehrenberg (in his " Corallenthiere des rothen Meeres)" was the first to include in it other species.

and he properly added to it the *G. verticillaris;* he describes also a third species under the name of *P. flabellum*, but I am not quite satisfied that this species is established. The locality given for *P. verticillaris* by Ellis and Solander, and by Marsigili, is the Mediterranean. I have good specimens of this species from the West Indies, where they were collected by the late Rev. Lansdown Guilding. I have met with no statement of the depth at which it grows, but have reason to believe that Mr Guilding did not use means to obtain his specimens (of which I have many) at great depths, and Ellis and Solander would certainly have told us if they had known of their occurrence at a great depth.

"*Primnoa lepadifera* is found, I believe, only on the coast of Norway. I have specimens nearly two feet in height, which were presented to me by Sir Arthur de Capell Brooke, Bart., who collected them there in 1820. He received accounts of their growing to a much larger size. They are found at great depths, varying from 150 to 300 fathoms. At these depths they grow in company with a large branching Alcyonium of a red colour (*A. arboreum*), and it is in fishing with lines for the red fish that the specimens are obtained. This fish frequents the places where these corals grow, and the lines getting entangled with the branches of the corals frequently bring up specimens of them, and sometimes the size and strength of the corals is such as to break the lines.

"The drawings represent four species,—*Primnoa Rossii*, fig. 1, of natural size; fig. 2, portion magnified; 3, 4, polype cells more magnified; 5, 6, 7, polype cell of *P. lepadifera*.

"The polype cells of Primnoa are membranaceous and covered with calcareous scales, the forms of which are accurately represented in the drawings, and are useful in forming specific characters, as they differ in form and number in each species. These scales do not cover the whole surface of the cells; on the inner side, next to the stem there is a part devoid of scales, as if, being less exposed, their protection was not needed. This is shewn in the figures of both species. The substance of the cells being membranaceous, they are movable in all directions, as is shewn by the different positions in which the cells of *P. lepadifera* have dried, and hence a mistake was made by Ellis, who described the cells as 'reflexed,' that is, with the mouth downwards, which must have arisen from the position in which his specimen had been hung up to dry, as the weight of the cells would make them fall.

"I will not go into further remarks respecting the different species, but have said thus much, because the Primnoas are a rare form of coral, and there is an additional interest in the Norway species occurring at a great depth, as is the case with your new species of the genus.* Very truly yours,

C. STOKES."

—*Sir James Clark Ross's Voyage to the Southern and Antarctic Regions*, vol. i., p. 334.

Hybridity in Animals and Plants, considered in reference to the question of the Unity of the Human Species. By SAMUEL GEORGE MORTON, M.D., Author of "Crania Americana," "Crania Ægyptiaca," &c.†

Part I. Mammalia.—Introductory Remarks.

The facts connected with hybridity in the inferior classes of animals, have an important bearing on one of the most interesting questions in Ethnography; and it is in reference to this question, that we now propose to arrange and review them.‡

It was taught by Buffon, John Hunter, and other naturalists of the past century, and is yet assumed by some learned men of the present day, that the hybrid offspring of two distinct species of animals, are incapable of reproducing their kind; thus making hybridity the test of specific character. It follows, according to this supposed law of nature, that if mankind embraced several species, the intermixture of these would go no further than to produce a steril hybrid variety. But since all the races are capable of producing, with each other, a progeny more or less fertile, it is inferred that they must all belong to one and the same species. This is the question at issue.

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* I learn from Professor Edward Forbes, that he has got from Captain Sullivan another new species of Primnoa from near Staten Land, in 278 fathoms. 1st August 1846.

† Read before the Academy of Natural Sciences of Philadelphia, Nov. 4 and 11, 1846.

‡ Dr James Cowles Prichard, the first Ethnographer of this or any age, has, with great care and candour, collected many of the following examples of hybridity, although to my view, they conflict strongly with his main position.—See *Researches into the Physical History of Mankind*, vol. i.

It may, at first view, appear superfluous to go over the whole ground of inquiry; but apart from its Ethnographic relations, it is my wish to call attention to a branch of science that has hitherto been singularly neglected, and perhaps more so than any other. Having sought in vain for some collective exposition of its details, I was at length induced to examine them for myself; and in now giving them publicity, I respectfully solicit, from practical observers, any authenticated examples of an analogous kind, that may not be embraced in this memoir.

We shall merely further premise, that naturalists have differed as to the import of the word *species*; but we know of no better definition than that which is expressed by "separate origin and distinctness of race, evinced by the constant transmission of some characteristic peculiarity of organization." The term *race* has been indefinitely and conveniently used in those instances in which it is difficult to decide whether an individual of any tribe of plants or animals, is a distinct species, or only a variety of some other species. Races are properly successions of individuals propagated from any given stock; and we agree with the learned Dr Prichard, from whom we cite these definitions, that when races can be proved to possess certain primordial distinctions, which have been transmitted unbroken, they should be regarded as true species.*

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* Researches into the Physical History of Mankind, 3d Edit., pp. 105, 109. —For some highly interesting views of this question, and their practical application, see Prof. Haldeman's Enumeration of Fresh Water Mollusca, in Boston Jour. of Nat. Hist., 1844. Further researches into Ethnographic affinities,

Let us now proceed to examine the question before us, commencing with the larger mammiferous animals, and proceeding from these to birds, fishes, insects, and plants.

Equine Hybrids.—The common mule, the progeny of the ass and mare, has been familiar to man since the days of Homer; and it is equally well known that with this animal, the hybrid form, as a general rule, begins and terminates. But the result appears to depend much on temperature; for in the south of Spain, mules have often been observed to produce young; and M. de la Malle observes that this phenomenon is frequent in hot climates, in which their period of gestation is twelve months, being the same as that of the mare. The same author quotes from Columella, the remark of Mago, a Carthagenian agriculturalist, that in his country the fecundity of the mule was a frequent event, although it was regarded as a prodigy in Greece and Italy. He adds, that these mixed mules do not cross again with each other, but only with the primitive species that has given them birth.*

The ancients gave the name *ginnus* to the offspring of the

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may render it probable that what are now termed the *five races* of men, would be more appropriately called *groups*; that each of these groups is again divisible into a greater or smaller number of primary races, each of which has expanded from an aboriginal nucleus or centre. Thus I conceive that there were several centres for the American group of races, of which the highest in the scale are the Toltecan nations, the lowest the Fuegians. Nor does this view conflict with the general principle, that all these nations and tribes have had, as I have elsewhere expressed it, a common origin; inasmuch as by this term is only meant an indigenous relation to the country they inhabit, and that collective identity of physical traits, mental and moral endowments, languages, &c., which characterize all the American races. The same remarks are applicable to all the other human races; but in the present infant state of Ethnographic science, the designation of these primitive centres is a task of equal delicacy and difficulty. I may here observe, that whenever I have ventured an opinion on this question, it has been in favour of the doctrine of *primeval diversities* among men,—an original adaptation of the several races to those varied circumstances of climate and locality, which, while congenial to the one are destructive to the other; and subsequent investigations have confirmed me in these views. See *Crania Americana*, p. 3; *Crania Ægyptica*, p. 37; and *Distinctive Characteristics of the Aboriginal Race of America*, p. 36.

* M. de la Malle, Ann. des Sciences Nat., xxvii, p. 235.

mule and the mare, which appears to have been a common animal among the Romans, who called it also the *Little Mule*, (*Parvum mulum.*)*

Prevost and Dumas, repeating the experiments of Lieuenhœck, assure us that the sterility of these mules in northern climates depends on an absence of spermatic animalcules; but the latter must be present in hot countries, to explain the phenomena of reproduction.

The *Hinny*, on the other hand, is the offspring of the horse and a female ass—*Bardo ex equo et assinâ*—an animal of a very refractory disposition, and little esteemed, either in ancient or modern times; nor have I been able to obtain any facts in relation to its reproductiveness.

Again, when a male, derived from the cross "between the she ass and the male *onager* (*Equus onager*), is allowed to couple with the mare, the offspring is more docile than either parent, and unites the beauty of form and gentle disposition of the father with the strength and swiftness of his grandsire;† whence the ancients preferred the onager to the ass for the production of mules, and Mr Gliddon informs me that this opinion is prevalent in Egypt at the present day.

The Baron Cuvier informs us that he had seen the cross between the ass and zebra, and between the female zebra and horse.‡

The ass is not the proximate species of the genus Equus, when compared with the horse; but that place is held, as Cuvier remarks, by the *Dziggetai* of Asia (*Eq. hemionus.*) And two distinguished naturalists, Mr Bell and Mr Gray, are even disposed to remove the ass to a separate genus. Without passing judgment on this question, I will merely observe, that in order to obtain a prolific breed of hybrid horses, the true horse should be coupled with the *hemionus*, under the same adaptation of climate and domesticity that have been bestowed on some other mixed animals; nor until this experiment has been fairly tried, can we speak with absolute certainty of the extent of productiveness of equine mules.

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* M. de la Malle, Ann. des Sciences Nat. xxvii., p. 143.

† Columella, quoted *ut supra*, p. 135.

‡ *Regne Animal, p. 182.*

The phenomenon of productiveness has little or no li among the true horses; whence it has been inferred that t all belong to one species; and that their various forms colours are solely owing to the diversified circumstance which they have been placed. But the researches of Ha ton Smith, have not only given rise to much doubt on t subject, but have adduced a surprising array of facts in vour of the opposite opinion.

We must refer to his learned and elaborate essay for t mass of evidence therein embodied; merely observing, on t present occasion, that he separates the horses into five p mitive stocks, which appear to constitute "distinct, tho osculating species, or at least races separated at so remot period, that they claim to have been divided from the e liest times of our present zoology."*

He adds, that some of these forms yet exist in the wi state on the table-lands of Central Asia, and that all of the were so constituted as to be fusible into a common, specifi but very variable stock, for the purposes of man; and h finally concludes, that if man had been necessitated to cult vate the zebras of South Africa, instead of the horses of Asia, he would have succeeded in amalgamating the three or four known species into one domestic animal, little inferior to the horse itself.†

It therefore becomes a reasonable supposition that some varieties of the horse now known to us may be hybrid mixtures of proximate species: more especially, since the facts collected by Hamilton Smith, De Azara, and De la Malle, shew conclusively that all the domestic horses were reclaimed from the original wild state.

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* Natural History of the Equidæ, p. 154.

† Ibid., pp. 75, 183. Fossil remains of the horse, and especially the teeth, have been of late years abundantly found in Europe and Asia, and in North and South America, and especially near Natchez, by Dr Dickerson; shewing that this animal was once indigenous to all these widely extended regions; and yet there are now no horses in the Western Hemisphere, excepting those that have descended from the European stock. The indigenous species must have become extinct from some remote and extended cataclysm. It is curious also to note that these fossil horses were different in species from the present variety, although they were closely allied to it. There were in ancient times several, *perhaps many*, species of the genus Equus.

Bovine Hybrids.—In the argument in question, the ox tribe has always been referred to as one of the strongest evidences of the operation of local causes in producing varieties of breed. But the parent type or stock is wholly unknown to naturalists; and although it corresponds in its osteological structure with a fossil species (*Bos urus*) found throughout Europe, it is extremely doubtful whether all the modifications now familiar to man are derived from this animal. "An opinion has lately been started," observes a learned zoologist, "that the haunched varieties of cattle are derived from a different species; against which no conclusive objection can well be made, when it is considered that the Gayal (*Bos gavæus*) produces a mixed race with the domestic animal; and that the yak of Tartary (*Bos grunniens*), and even the American bison, are equally reported to mix with that species, notwithstanding their anatomical differences, and that the times of gestation are not similar."*

The hybrid offspring of the buffalo and the common breed of cattle is now familiar in the western parts of the United States, particularly in Missouri and Kentucky; but I have not yet ascertained whether they have ever bred again among themselves, or with either of the parent stocks.† I have instituted inquiries on this subject, the results of which I hope to add as a sequel to this memoir. In fact, it is now conceded that all the species of the genus Bos are similarly circumstanced;‡ whence we have no difficulty in supposing that among the ox tribe, as among various other classes of animals, hybridity has more or less modified their forms during the long lapse of thousands of years.

Bovine and Cervine Hybrid?—The Baron Larrey incidentally mentions, in his Memoirs, the following circumstance that occurred during his residence at the Bay de Croc, in Newfoundland; "The *Carabou* (*Cervus Wapiti*) sometimes comes near the houses. In the night, one of them broke into our sheep-fold, where we had a cow, that became pregnant by him. She no doubt produced a mongrel; but I lost the opportuity of ascertaining the fact, because she was taken back to Brest."*

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* Griffith's Cuvier, iv., p. 419.—Prichard's Researches, i., p. 141.

† Desmoulins, however, speaks positively in the affirmative.—Hist. Nat. des Races Humaines, p. 195.

‡ Loudon's Magazine of Nat. Hist., ix., p. 511.

I see no reason to question any part of this statement, which ceases to astonish us when we regard the many analogous phenomena that are now fully authenticated, and among others the following very remarkable one.

Bovine and Ovine Hybrid.—In Brande's Dictionary of Literature and Science,† it is mentioned, without doubt or reservation, that a mule has been obtained between the bull and sheep; a statement that claims our entire credence, from the circumstance that the physiological part of the work in which it is contained, is from the pen of Prof. Owen, of the Royal College of Surgeons.

Cervine Hybrids.—The only example of this class that I have met with in authors, is that between the Indian buck of the Axine species (*Cervus axis*) with the Porcine species, thus giving rise to the well-known intermediate stock, called the *Spotted Hog-deer.*‡

Caprine Hybrids.—The goat called the *Wild ægagrus*, which is found in all the alpine regions of Europe and Asia, appears in every instance to be a prolific hybrid between the domestic goat and the local wild one of the country it inhabits, although the latter animal may be the *Ibex*, the *Caucasica*, or any other species.§

A mixed breed has also been obtained between the chamois (*Antilope rupicapra*) and the common goat.‖

Ovine Hybrids.—It was until lately supposed by most zoologists, that the domestic sheep, and the Asiatic and American *Argali*, were mere varieties of one species; but they are now known to be distinct, and are severally designated by the names of *Ovis musmon*, *O. ammon*, and *O. pygargus*. The common sheep, called in the systems *O. aries*, is generally classed as a variety of the first named species; but recent investigations render it more than probable that several wild species have commingled to form the numerous cultivated races.[*]

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* Memoirs of Military Surgery, &c., Dr Hall's trans., vol. i., p. 11.

† Article *Hybrid.*

‡ Hamilton Smith, Æquidæ, p. 341. § Idem. ‖ Idem.

Ovine and Caprine Hybrids.—The ancients, more especially the Romans, regarded all the varieties of domestic sheep as a mixed offspring of the sheep and goat (*Capra hircus*). The possibility of this union was proved by Prof. Pallas, by personal observation during his travels in Russia; and although a doubt has been here and there expressed with respect to it, the fact is now conceded by all naturalists, from abundant evidence. Some new and very interesting information has lately been afforded us from another quarter. "For a very long time," observes M. Chevreul, "an extensive commerce has been carried on in Chili, in the skins of sheep with rather coarse wool, derived from a cross between the male of the common goat and the ewe, which was obtained as follows: a single goat was placed with six ewes, and male hybrids were obtained with a hairy fleece, which was little esteemed for the particular purpose for which it was designed. But by coupling these male hybrids with ewes, the latter were fruitful, and their offspring bore a fine soft fleece, which is highly valued in the manufacture of *shabraques*, called also *pellians*, in Chili. After several generations, the hair becomes coarse and hard, when it becomes necessary to recur to a male hybrid of a former generation, in order to obtain the requisite cross for the production of the perfect fleece.[†]

I have only to add, on the same authority, that Prof. Flourens, of Paris, has recently obtained a cross between the wild ram[‡] (*Ovis musmon?*) and the female of the common goat.

Cervine and Ovine Hybrid.—Hellenius, quoted by Rudolphi, mentions the very interesting case of a Sardinian doe that refused the goat, but was crossed by a ram. The young had the figure of the father, but in colour more resembled the mother. These hybrids were again crossed by a Finland ram, and after a few generations assumed the characters of the Finland breed of sheep.[§]

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[*] Hamilton Smith, Æquidæ, p. 70.—Blyth, Proceedings of the Zoolog. Soc. of London, 1840.

[†] Journal des Savants, Juin 1846, p. 357.

[‡] Moufflon. The particular species is not designated.

[§] Rudolphi, Beyträge zur Anthropologie, &c., p. 165.

Cameline Hybrids.—The two species of camel, *C. bactrianus* and *C. dromedarius*, produce with each other an intermediate offspring, which is said to be fertile without limit. Buffon could not deny this proverbial fact; and in order to obviate a difficulty that conflicted with a favourite opinion, he assumed that these animals must be mere varieties of a single species. Modern science, however, has established, beyond question, the specific difference of the camel and the dromedary.*

Canine Hybrids.—If we could admit that all the dogs, with their varied external forms and peculiar instincts, have been derived from a single pair of these animals, we could have no difficulty, I conceive, in adopting so much of Lamarck's theory as relates to the progressive transmutation of species, resulting from what he calls the force of external circumstances; and it is curious to observe that he especially adduces the canine race in support of his hypothesis. "In nature we seek in vain for mastiffs, harriers, spaniels, greyhounds, and other races between which the differences are so great that they *would be readily admitted as specific* among wild animals; yet all these have sprung originally from a single race, at first approaching very near to a wolf; if, indeed, the wolf be not the true type which at some period or other was domesticated by man."†

He further maintains that the peculiar instincts and functions of animals,—the dogs for example,—have not resulted from a previous and pre-adapted organization; but that these instincts, on the contrary, have developed, by constant use, those very organs of which they are the seat. The greyhound, for example, has derived his long and slender legs, and his proverbial speed, from the mere habit of running with celerity in pursuing some animals and in escaping from others. The mastiff again has become large, strong, and muscular, from habitually seizing and holding animals larger and stronger than himself. In fine, Lamarck applies the same principle to all organized beings, which, according to his doctrine, have been developed by the mere force of circumstances,—a tendency to progressive advancement from the simplest to the most perfect forms. And here we may inquire,—If education and domesticity can so vary not only the instincts but the very proportions of anatomical structure in dogs, do we not realize in the theory of Lamarck, a law of nature which would, with equal readiness, explain the unlimited transmutation of species into each other?

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* Cuvier, Regne Animal, vol. i., p. 187.

† See Lyell's Principles of Geology, B. III., chap. 1, &c.

But is it proved that all the domestic dogs are really derived from a single species? Here again we appeal to one of the latest and best authorities on this question—Charles Hamilton Smith, whose laborious researches have led him to the following conclusions—that the parents of our domestic dogs are derived from several distinct species, which were constituted with faculties to intermix, and thus to produce the interminable varieties familiar to man; that five of these types belong to the Old World alone, viz., the *wolf*, the *buansu*, the *anthus*, the *dingo*, and the *jackal*; that a *dhole* or a *thus* may have been the progenitor of the greyhound; and that the origin of the primitive mastiff may yet be traced to a lost or undiscovered species belonging to the hyena tribe.*

The wolf, the dog, the jackal, and the fox, all intermix with each other. So does the common jackal with the jackal of Senegal. Do they therefore belong to one species? It is well known that the cross between the dog and the European wolf in the experiments of Buffon, did not extend beyond the fourth generation; but the distinguished writer whom we have just quoted, has observed, that the animals were in a state of neglect and restraint, and gradually tended to sterility, from their small number, and from the want of recrossings from one or other of the parental stocks. It is worthy of remark, that the *dingo* of Australia when placed in similar circumstances with the common dog, also becomes steril in the fourth generation; whence, according to this test, the *dingo* is not a true dog, but some other species of the genus Canis.

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* Natural History of the Dog, in Naturalist's Library, vol. i., p. 104, *et passim*. The *Canis venatica* of Burchell, connects the dog with the hyena almost without an interval.

The greatest number of mammæ in the common dog is ten, the smallest number, six; in the wild species they are always in pairs, and they never vary in a species. "To what other cause, then, can we ascribe the anomaly in domestic dogs, so justly as to an intermixture of species?"*

The dogs that have become wild in Paraguay, always hunt in packs, thus resuming the wolf-life instinct of their progenitors. Will it be said that this is a newly developed instinct? or is it not rather an old one that new wants have reproduced.

It is therefore certain that dissimilar species of the dog tribe are capable of producing a fertile hybrid offspring; and if it was the interest of man again to cultivate and extend these mixed species, there is every probability that the race would become unlimited.

"Experiments shew," observes Mr Lyell, "that after repeated failures, the union of two recognized species may at last, under very favourable circumstances, give birth to fertile progeny; and such circumstances," he adds, "the naturalist may conceive to have occurred again and again in the course of a great lapse of ages."†

Every one who is in the least degree acquainted with the natural history of dogs, knows that certain remarkable changes of colour, and sometimes of form, take place in particular localities. These changes are usually attributed solely to climate, food, training, and other exterior agents. I do not deny the modifying action of such agents in these and other cases; but it is a reasonable subject of inquiry whether there may not be something in these localities that favours an effort of nature to reproduce a primitive type? The localities to which we allude,‡ do not operate equally on all varieties of the dog tribe; which we might suppose would be the case if all the canine breeds were derived from a single stock or species. It is important, in connection with this subject, to observe that all the pure Indian dogs of

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* Nat. Hist. of the Dog, in Nat. Lib., vol. ii., p. 79.

† Principles of Geology, Book III., chap. 2.

‡ See Dr Prichard's Natural History of Man, for an admirable exposition of these and all other facts on which the *analogical argument* is founded.

h America are of one variety, with erect ears, a wolfish
et, and having a howl in place of a bark. Most natu-
ts agree in considering it a reclaimed wolf. The late
Thomas Say, regarded it as the *Canis latrans* or *Howling*
in a state of domestication. It is remarkable, when
xed, for the uniformity of its characters, which are the
in every locality over thousands of miles in extent.*
arieties have arisen from it, excepting by crossing the
l with other dogs, when a hybrid is produced that is pro-
vithout end. It is much to be regretted that so little
own of the history of the indigenous dogs of America,
ubject that affords a fine field for scientific inquiry.

hile engaged in writing this memoir, I am assured by
iend Dr M'Coy, an intelligent physician and naturalist,
in the interior of Pennsylvania, the common wolf, *C.*
, has been taken when young, and successfully trained
er hunting. The difficulty, however, with these animals
that they devoured the game, unless the sportsman was
e spot to prevent them. To obviate this fault, these
es were crossed by the common dog; giving rise to a
d breed, that combined the keener instinct of the wolf
the greater docility of the dog. Should these hybrids
duce among themselves, or with either of the parental
es, how completely will the history of these animals il-
ate the origin of the dog tribe, its primitive domestica-
the crosses between different species, and the varieties
must have followed from such intermixture? I hope yet
able to lay before the reader all the facts of this singular
ry.

rine Hybrids.—Another domestic animal which presents
rkable varieties of form as well as of marking, is the
and these have also been attributed to a single species
fied by immemorial domestication. Some new light,
ver, has recently been thrown on this branch of zoology
r Eyton of London, who has compared the skeletons of

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arver's Travels in North America, p. 417. See also the plates of the
ficent Atlas of the Prince de Wied's Travels in this country.

the Chinese, the African,* and the English pig, and finds, that while they agree in the number of cervical vertebræ (as indeed all quadrupeds do), there is a remarkable difference in each of the other classes of these bones. We have not space for details, except to observe that the dorsal vertebræ vary from thirteen to fifteen, the lumbar from four to six, and the caudal from thirteen to twenty. Now, as far as time and circumstances had allowed the experiment to proceed, these several animals bred freely with each other, and in the instance of the Chinese pig, the offspring is unquestionably fruitful.

Mr Eyton very justly remarks, that the above three pigs must be considered as distinct species, or osteological characters can no longer be received as criteria of species; and Hamilton Smith has arrived at the conclusion, that there were three, if not four, original species, endued with powers of unlimited reproduction.

Feline Hybrids.—These animals, at least the domestic varieties, had long been regarded as of one species; but modern researches have established that the blue or Chartreuse cat, originally belonged to a distinct feline group; the Bengal cat of Pennant pertains to a second; while the tortoise-shell cat is believed to have sprung from a third group originally indigenous to South America†. I believe all these animals produce with each other a fertile offspring. It may be denied, however, that they belong to different species; but that the domestic cat was once of at least two species, seems now decided by the observation of Dr Ruppell, who finds the embalmed cat of the Egyptians to correspond to the *Felis maniculata*‡ of Nubia, and not to the *Felis domestica*. Where, then, is the race of cats once so abundant in ancient Egypt?

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* The *Sus æthiopicus* has even been removed to a separate genus by Cuvier —*Phascochœres*. See *P. Æliani* in Ruppel, Atlas zu der Reise in Nord-Afrika, p. 61.

† Hamilton Smith, Equidæ, p. 339.

‡ Atlas zu der Reise im Nordlichen Afrika, p. 4, tab. 1. Prof. Bell has also decided that the *Felis catus*, found wild in the forests of Europe, is different from both the domestic species.

They have probably come down to us so blended with other species that their identity is lost.

De Azara states, that in the forests of Paraguay the *Felis yaguarundi* and the *Felis eyra*, both unite with the domestic cat; and he adds, that should these wild species become in time extirpated, and the mixed breed alone remain, the latter would be very naturally referred with all its varieties, to a single original species.*

Mixed breeds have also been obtained between the black leopard and the African species, and between the lion and the tigress. The latter cross which is much the more remarkable, produced three cubs, which were doing well at the time the facts were published.† We regret that no further particulars have come under our notice.

Feline and Musteline Hybrid.—A most remarkable instance of hybridity between the cat and an animal of a totally distinct genus, is described in the following account, which is published in several of the best scientific periodicals, and appears to be well authenticated. "A domestic cat disappeared from a house in Penza. After being absent some time, she returned; and within the regular time, produced four young ones, two of which strongly resembled the marten. Their claws were not retractile, as in the cat; and the snout was elongated, like that of the pine marten, (*Mustela martes.*) The two others of the same litter more nearly resembled the cat; as they had retractile claws and round heads. All of them had the black feet, tail and ears of the marten; and they killed birds and small animals more for the pleasure of destroying them than for food. The proprietor endeavoured to multiply this race, and to prevent their intermixing with the domestic cats, in which he proved highly successful. In the space of a few years he reared more than a hundred of these animals. A specimen presented to the Imperial Society of Natural History of Moscow, was of the third or fourth generation, and it retained all the characters of the first."‡

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* Quadrupeds of Paraguay, i., p. 174.

† *Vide* Griffith's Cuvier, ii., p. 448. 1827.

‡ Loudon's Mag. of Natural History, ix., p. 616; Griffith's Cuvier, ii., p. 489.

Professor Pallas has described and figured the *Pensa cat*, which has long been suspected for a hybrid, although very prolific. It may yet prove to be the animal we have just described.

Lepine Hybrid.—Amoretti, quoted by Rudolphi, has published the history of a cross between the European or English rabbit, *Lepus cuniculus*, and the hare *L. timidus.*[*]

Phocine Hybrid.—Finally, among mammiferous animals, it remains to notice the singular fact discovered by the traveller Steller, and mentioned by Rudolphi, that the sea-lion, *Phoca jubata*, of Behring's island, produces young with the sea-bear, *P. ursina.* " I have no doubt of this fact," adds Professor Rudolphi, " since Pallas speaks of Steller with the greatest respect, and Telesius proved the accuracy of his observations."[†]

Part II.—Birds.

Gallinaceous Hybrids.—The variation of size, form and plumage, so remarkable among the different breeds of domestic fowls, has been usually attributed to the action of physical agents on a single original species. This supposition, however, is now found to be untenable; for the best ornithologists, and those, too, who have no view to collateral generalization, have succeeded in tracing this family of birds to at least ten different species. Without appealing to unnecessary details it is sufficient to observe, that independently of certain admitted changes as the result of domestication, these birds are in far greater degree modified by the power possessed by their several species (so far, at least, as the experiment has been extended), of mingling with each other and producing a fertile hybrid progeny. Hence, in a great measure, those interminable varieties of exterior form, size, and colour now everywhere familiar.[‡]

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[*] Rudolphi, Beyträge zur Anthropologie, &c., p. 165.

[†] Ibid. *Loco citat.*—Prichard's Researches, i., p. 142.

[‡] We have been at some pains to ascertain the specific names of the several *original* gallinaceous birds; yet without presuming that ornithologists have described all that exist, we append a list of such as are already known: *Gallus*

The *Gallus ecaudatus* (tail-less fowl) has been triumphantly quoted as an evidence of the power of climate and locality to produce changes, not only of plumage, but of anatomical conformation. This bird is deficient in the last dorsal vertebræ, and consequently has no tail. But it was asserted, even by some naturalists, that this fowl was originally possessed of a tail, but lost it on being sent from England to Virginia, and domesticated in the latter country. More recent investigations, however, have proved, that this is a wild native species of Ceylon.*

The fowl with rumpled or inverted feathers, which was long regarded as a mere accidental variety, is now believed to be a distinct species, and a native of Guiana. It breeds with all the other domestic fowls, and the offspring is prolific without end.

Fortunately for the further elucidation of this question, the continent of America produces a family of gallinaceous birds, —the *Alectors* of ornithologists,—among which the very same intermixture of species takes place, and consequent fertile offspring, as we have remarked in the several species of domestic fowls. All the *Hoccos* or Currasows (*Crax*) for example, which are derived from their native forests of Guiana, readily unite with each other, giving rise to a progeny that is reproductive without end. "It is probable," observes a judicious ornithologist, "that if the intercourse were repeated in a variety of ways, it would be possible to cultivate, by suitable care, many different races of these birds, whose descendants might be susceptible of multiplying *ad infinitum*, and branching out into a number of singular varieties, under the superintendence of man."†

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bankiva ; G. æneus ; G. Anstrutheri ; G. furcatus ; G. Souneratii ; G. Lafayettii ; G. giganteus ; G. morio ; G. lanatus ; G. ecaudatus, and *G. crispus*. I omit several supposed species, which may, for the present, be regarded as apocryphal. I have had the pleasure of examining *five* of these original species, contained in the magnificent *Wilson Collection of Birds*, now deposited, by its liberal proprietor, in the Academy of Natural Sciences of Philadelphia.

* Griffith's Cuvier, viii., pp. 19, 21, 173 ; Temminck, as quoted in the same work.

† Griffith's *Cuvier, viii., p. 100.*

ent characteristics were owing to a mere difference of locality.*

On the other hand, those persons who, for pastime, have given great attention to this subject, are of the opinion that the cage-canary is not derived only from the *Fringilla canaria*, but that it is, itself, a fertile hybrid, of which there now exist upwards of thirty various breeds.

Syme, who has written on the British song-birds, says that the wild canaries of the islands that bear that name, have less resemblance in song and plumage to the domestic canary, than to the *Siskin* of Germany, the *Venturon* of Italy, or the *Serin* of France.

The celebrated ornithologist Bechstein adds the following remarks: "We might almost conclude that the *Venturon* (*F. citrinella*), the *Serin* (*F. serinus*), and the *Siskin* (*Carduelis spinus*), are the wild originals of the cage-canary. I have seen a bird produced between the *Siskin* and *Serin* which perfectly resembled the variety called the green canary;—I have also seen a mule from a grey female canary, whose true parentage could not be distinguished." The Siskin, it will be observed, belongs to a different genus from the wild canary.

The canary is now known to breed, not only with the three species just mentioned, but also with the linnet, sparrow, chaffinch, bunting, greenfinch, and bullfinch; and with several of them it produces a fertile offspring.

M. Veilliot once caught a mule bird, which he supposed to be the produce of a male greenfinch (*F. chloris*) and a female goldfinch; for it mingled the size, colour, and song of both these species. This bird did not appear to have resulted from the domestication of the parents, for it remained extremely wild, yet was brought to couple with a female canary.†

A yet more remote alliance, that between a canary and a nightingale, produced an egg that could not be hatched. This fact conjoined with others to which we have adverted, illustrates the remark of Professor Temminck, that the ocnce of prolific offspring between different species of birds, evidence of the near affinity between them; and that, the reverse takes place (infertile mules), it proves the rity between the species thus brought together.

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* Griffith's Cuvier, vii. 271.

† Idem, p. 259.

brids of the Anatidæ.—The cross between the *Anser ides* and the common tame goose of Europe, is prover- frequent, and the offspring has proved prolific.* Mr and Mr Blyth have recorded examples of this kind, I. Chevreul has seen the progeny extend through seven ations.†

Anser canadensis is often taken in the United States educed to the domestic state, in which it crosses with mmon goose, producing a hybrid offspring, which, how- appears to be steril.

swan (*Cygnus olor*) has crossed with the common ean goose, but I am not aware that the hybridity has noticed beyond the first offspring. This fact is quoted Prichard from M. Frederick Cuvier.

Charles Waterton, the celebrated traveller, has pub- a very interesting account of a hybrid between the fe- Canada goose and the wild Bernacle gander (*Anser la*).‡ This fact occurred at Mr Waterton's seat at n Hall, England. "These hybrids," he observes, "are tly shaped, but are not so large as the mother, nor so as the father, their plumage partaking in colour with f both parents. The white on their front is only half ch as is seen on the front of the gander, while their are brown in lieu of the coal-black colour which ap- on the neck of the goose. Their breasts, too, are of a colour, while the breast of the bernacle is black, and f the Canadian white; and throughout the whole of the ing plumage there may be seen an altered and modi- louring, not to be traced in that of the parent birds."§ emains to notice some instances of hybridity among

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don's Magazine, ix., p. 511. Temminck, Manuel d'Ornithologie, i., -Eyton, Monograph of Anatidæ.

rnal des Savans, June 1846.

nicla brenta.

§ Essays on Nat. History, p. 118, 2d edition.

different species of ducks; thus a cross has been obtained between the *Anas fuligula* of Europe, and the European teal (*Anas querquedula*).[*] "Selby mentions a male wigeon (*Mareca penelope*) breeding with a female pintail (*Anas acuta*), notwithstanding the fact of females of his own species being kept on the same piece of water." Other crosses have taken place between the pintail and the common mallard (*Anas boschas*);[†] and, in the wild state, between the latter and the dusky duck (*A. obscura*), of which my friend, Mr William Gambel, has seen a specimen in the possession of Mr J. G. Bell of New York, near which city it was shot. A much more frequent hybrid occurs between the mallard and muscovy duck (*A. moschata*).

Mr William R. Clapp informs me that he saw at Rye Pond, in the state of New York, a female wild duck that had been taken when young and placed in company with the common tame species, and that a fertile hybrid progeny was obtained from them. From the description of this wild bird, I suppose it to have been the *Anas rurfiterques.*

Other Hybrid Birds.—Among bird-hybrids it only remains to notice those of *Motacilla lugubris* and *M. alba*, as recorded by Temminck, and which possessed colour and markings intermediate between both parents.[‡] A mixed breed has been repeatedly observed in Scotland between the common crow (*Corvus corone*), and the hooded crow (*C. cornix*), and another mixed species between the latter bird and the carrion crow. Finally, a hybrid has been obtained between the throstle (*Turdus musicus*) and the blackbird (*Merula vulgaris*) of Great Britain.[§]

Part III.—Reptiles, Fishes, Mollusca, and Insects.

Among reptiles, I have found but a single authenticated example, that of a cross between the European frog and toad, which are generally distinct. (*Bufo* and *Rana*.)[||]

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[*] Prichard. Researches, &c., i., p. 140, from M. Geoffroy St Hilaire.

[†] Loudon's Magazine, ix., p. 615.

[‡] Manuel d'Ornithologie, i., p. 254.

[§] Loudon's Magazine, ix., p. 615.

[||] Brande's Dict. of Science, &c., Art. Hybrid.

Among fishes, specific hybrids have been obtained by means of artificial impregnation between *Cyprinus carpio* and *C. carassias,* and between the former species and *C. gibellio.** "Defay mentions a hybrid between the *barbus* and *carpio,* and Bloch a similar production intermediate between *Cyprinus blicco* and *C. brama.*"†

My friend Mr S. S. Haldeman, well known for his many and accurate contributions to various branches of natural science, has kindly furnished me with the following note in relation to some American freshwater Mollusca, in connexion with the present enquiry.

"Whilst I deem *Unio radiatus* and *U. siliquoideus* distinct species, there is a certain variety, apparently of the latter, although almost precisely intermediate, which induces me to believe it may possibly be a hybrid. I have never, however, seen any thing like a hybrid between the other species inhabiting the eastern waters of the United States, although I have had many opportunities for observation.

"In their proper localities, no species can be more readily separated than *Paludina decisa* and *P. ponderosa;* yet intermediate individuals occur, which it is extremely difficult if not impossible to give an undoubted place with either. This difficulty has sometimes almost induced me to regard the two as one species; but when I found that the best developed specimens of *P. decisa* never take the form of *P. ponderosa* where the latter is not found, as in the waters east of the Alleghanies, I could not safely unite them. Perhaps hybridity may be the cause of the difficulty where the two species occur in the same locality."

In Entomology, Mr Haworth has published some highly interesting facts relating to hybrid productions, and particularly in respect to the genus *Coccinella.* Fabricius had noticed and published some of these facts; but Dr Prichard remarks, that they "do not afford an equivocal example of the union of different species, since, according to the opinion of Illiger, accidental varieties of the Coccinella have been frequently mistaken for distinct kinds."* Yet, on the other hand, Mr Haworth asserts that he has often seen, *in coitû*, several different species of this genus; and he adds the following observations:—

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* Brande's Dict. of Science, &c., Art. Hybrid.

† *Prichard. Researches,* &c., i, p. 140.

"That they mix sexually with each other, when their proper mates cannot be found, is well known; and I have even had *larvæ* produced from the union of *C. tripunctata* and *C. quadripustulata*, which, but for an accident, would have been reared. Yet such junctions cannot destroy the distinctions of the primitive species, although it may give birth occasionally to hybrid broods; not barren, but capable of generating, for a while, others like themselves. Such, in all probability, are *Coccinella annulata* and *C. fasciata*. If these two had never existed, no entomologist would have conceived that all the insects of this section and *C. bipunctata* were of the same species! Wherefore it follows that they are not.

"Practical entomologists well know that similar unions happen in other genera, but more especially in *Cicada*; and from them arise occasionally a set of hybrid varieties, which still do not overturn the primitive distinctions of the original species whence they sprung, however difficult they may sometimes render the task of discriminating amongst such a set of mongrel productions.

"It is even probable that two species of distinct sections may occasionally generate a race very different from both parents, yet resembling both, and not barren, as is usually the case with mules, but capable of procreating. And such a brood some hold to be a new and distinct species in the scale of nature—brought to light by her own operations, and in the very same way that she has occasionally multiplied, and still continues to increase, the stupendous members of the vegetable kingdom."†

Part IV.—Plants.

Dr Prichard, as the result of extensive inquiry, informs us that the number of hybrid plants in the wild or uncultivated state, is about forty; that a few of these have reproduced, but that the greater part of them are steril.* On the other hand, it is asserted by Shiek, that a multitude of plants produce specifical hybrids in a state of nature.†

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* *Ut supra.*

† Trans. of the Entomological Soc. of London, i., pp. 267, 291.

There are innumerable instances, as every one knows, of crosses obtained from plants of different species of the same genera, even when brought from the most distant parts of the world, as the experiments of Kolreuter, Sagaret, and Herbert, abundantly testify. Not only are these hybrids fertile, but in some instances their reproductiveness exceeds that of the parent plants, by multiplying not only from the seed, but from roots, shoots, and suckers. The intermixture is not confined to particular species, but even the most dissimilar can be crossed. We think it unnecessary to give examples when the facts are available to every one; and therefore, in respect to the blending of *species* among plants, the reader is particularly referred to the admirable essays of the Rev. Mr Herbert and M. Sagaret.‡

We may remark, however, that so abundant are these hybrids, that Mr Herbert, in order to avoid the difficulty they present to a favourite theory, declares it as his opinion, that botanical species are only a higher and more permanent class of varieties, which should be discarded from the systems, leaving the *genera* to define the individuality of kind; or, in other words, to designate those permanent characteristics which have hitherto, in his opinion, been erroneously attributed to species.§

But, in the treatise of M. Sagaret, various instances are given of hybrid plants derived from the mixture of different genera; thus realizing, in this department of nature, the same facts that we have seen to occur in the several sections of the zoological series. We will offer a single example,—the cross between the horse-radish (*Cochlearia raphanus*) and the cabbage; the former bearing a short pod or *silicula*, the latter a long pod or *siliqua*.

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* Researches, &c., i., p. 139.

† Brande's Dict. of Science, Art. Hybrid.

‡ Herbert. Amaryllidaceæ. *Introd.*—Sagaret. Annales des Sciences Nat., T. viii.

§ Amaryllidaceæ, p. 19.

Remarks.

While we admit that hybrids, as a general law, are contrary to nature, we are also compelled to concede that this law has very many exceptions. "It is manifest," says Dr Prichard, "that there is some principle in nature which prevents the intermixture of species, and maintains the order and variety of the animal creation. If different species mixed their breed, and hybrid races were often propagated, the animal world would present a scene of confusion. By what method is this confusion prevented? The fact seems to be, that the tribes of wild animals are preserved distinct, not only by the sterility of mules, but that such animals are never, in the state of nature, brought into existence. The separation of distinct species is sufficiently provided for by the *natural repugnance* between individuals of different kinds. This is, indeed, overcome *in the state of domestication*, in which the natural propensities cease, in a great measure, to direct their actions.*

But we have seen that mules are not always steril, and also that hybrids are really produced in a state of nature, wholly independent of the influence of cultivation; facts which, indeed, are admitted and illustrated by Dr Prichard in his later writings. That domestication evolves the faculty of hybridity there can be no question; and we would apply the principle to various classes of animals. It will materially assist in explaining so great a variety in some animals, by pointing, as De Azara and Hamilton Smith have suggested, to certain primitive species, which were endowed with the capacity for reproducing among themselves, especially under the influence of domestic culture. We have shewn that this fact is unquestionable among some quadrupeds and some birds, of which the hybrid varieties have been cultivated for the uses of man.

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* Researches into the Physical History of Mankind, i., p. 97. *Second Edition.*

Could we trace back the origin and history of various other species, we should, in all probability, arrive at the very same result; for it appears to be a law of nature, that the faculty possessed by different species of animals of producing fertile hybrid offspring, is in proportion to their aptitude for domesticity.

Now, since man possesses this aptitude in the highest degree, being, as Blumenbach expresses it, the most domestic of animals, it would be nothing singular if he possessed the power of fertile hybridity, even if the human family should prove to embrace several distinct species; because, as we have fully shewn, this phenomenon is not unfrequent among animals whose specific, and even generic diversities, are unquestionable. If, therefore, domestication, or, as we have termed it, the aptitude for domesticity, explains the fact in one instance, it certainly does so in the other, more especially since fertile reproduction has ceased to be evidence of identity of species.

A word with respect to the theory of *repugnance*. The same phenomena, moral as well as physical, take place, to a certain extent, among men as among animals; for the repugnance of some human races to mix with others, has only been partially overcome by centuries of proximity, and, above all other means, by the moral degradation consequent to the state of slavery. Not only is this repugnance proverbial among all nations of the European stock among whom negroes have been introduced, but it appears to be almost equally natural to the Africans in their own country, towards such Europeans as have been thrown among them; for with the former a white skin is not more admired than a black one is with us.*

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* See the travels of Hawkins, Browne, Burkhardt, Caillet, &c., for abundant evidence of this fact.

I must here be permitted to offer a single additional remark. It is obvious that while cultivation produces obvious changes in some animals, its influence has had little or no effect on others; for example, the ass, the rat, and the mouse, among quadrupeds, and the peacock and guinea-fowl among birds. These species have been domesticated from immemorial time, in all latitudes, under every conceivable variety of circumstances. Among wild birds and quadrupeds, on

Conclusions.

1. A latent power of hybridity exists in many animals in the wild state, in which state, also, hybrids are sometimes produced.

2. Hybridity occurs not only among different species, but among different genera; and the cross-breeds have been prolific in both cases.

3. Domestication does not cause this faculty, but merely evolves it.

4. The capacity for fertile hybridity, *cæteribus paribus*, exists in animals in proportion to their aptitude for domesticity and cultivation.

5. Since various different species of animals are capable of producing together a prolific hybrid offspring, hybridity ceases to be a test of specific affiliation.

6. Consequently, the mere fact that the several races of mankind produce with each other a more or less fertile progeny, constitutes, in itself, no proof of the unity of the human species.

A Discourse. By JOHN DAVY, M.D., F.R.S., Inspector-General of Army Hospitals, Honorary Member of the General Agricultural Society of Barbadoes. Read at their Third Half-Yearly Meeting in 1847.

GENTLEMEN,—In the discourse I am about to have the honour to address to you, it is my intention to bring under your notice the subject of manures. To you, as practical agriculturists, I need not insist on, or endeavour to point

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the other hand, some undergo very remarkable changes in a state of nature, as some species of squirrel, fox, wolf, &c., while other species of the very same genera undergo no change whatever. Hence the fallacy of drawing inferential conclusions from those that *do* change, in order to explain the phenomena of diversity among men.

The diversities of animals are in some cases owing to exterior causes alone; in other instances they arise solely from amalgamation of species; while in a third class we can trace the operation of both these agents.

out, its importance; one word may suffice to convey an idea of it, and even an adequate idea, viz., that manures are the *food* of plants. And this definition suggests the analogy that exists between plants and animals, and which I shall dwell on for a moment, and revert to, because I believe it to be instructive, or fitted to bring clearly to the mind some general views, which may be an aid in discussing the subject, considered scientifically, that is, as regards principles, or general rules deduced from facts or experience, and themselves applicable to practice.

Plants and animals have in common the distinctive property of reproduction, a power exercised by means either of a bud, slip, seed, or ovum; the seed of one being analogous to the ovum of the other; whilst the bud or slip-manner of generation are common to both, and constitute one of their most remarkable links. Having a common mode of origin, so have they of growth; as the animal grows, not like the mineral from accretion from without, but by deposition from within, so likewise does the plant. Both plants and animals are nourished, and owe their growth to foreign matter introduced from without; and both cease to grow—both waste and ultimately perish, if the foreign matter constituting their food be withdrawn. To both, warmth, light, air, and moisture, are, in certain degrees, essential to their well-being; and to both, in other degrees, these are injurious. Whilst there are thus certain resemblances between plants and animals, there are also marked and characteristic differences. The two most remarkable are intimately connected with the subject under consideration,—the kind of food required by each, and the kind of organs belonging to each for its reception. A mouth and stomach appear to be essential to the animal, in which the food taken is prepared, more or less, for distribution and nourishment. In the plant the preparation appears to be external—in the soil, from whence the nutritive fluid is absorbed by the delicate roots, and by them conveyed for distribution where required. Asto food, animals are dependent for their support on one another, or on vegetables. Plants, on the contrary, are not so dependent; they derive their support from the soil and from the atmosphere; and whilst animals, in the act of supporting themselves, convert organic into inorganic matter, vegetables, in their growth, have the opposite effect; they create or form organic from inorganic materials,—are, in brief, organizers for the sustentation of animal life. Let us take an example—A single seed of Guinea corn, weighing about a quarter of a grain, planted in an artificial soil, composed of several earths, and containing a little phosphate of lime, and salts of the vegetable and volatile alkali, under favourable circumstances, with sufficiency of moisture from rain, will rapidly vegetate, give rise to a plant many feet in height, and in less than six months yield a ripe head of corn, weighing, in its dry state, 1685 grains, and containing 3537 grains of seed; for such I have found to be the weight of a head of average size, and such the number of the seed it contained; the weight of the seed alone was 1460 grains. What a vast increase is here! And, if we examine the parts of the plant, its roots, its stem, its leaves, its seed, we shall find them composed of substances differing altogether from the materials which had constituted the food of the plant, a difference depending on a new combination of elements from, in brief, inorganic to organic compounds.

There is another point of difference, and a very interesting one, between plants and animals,—the effect they have on the atmosphere, comparing the leaves of the one with the lungs of the other. Animals inhale common air, consisting of azote and oxygen; a portion of the latter disappears, and its place is supplied by carbonic acid, which is a compound of carbon and oxygen, and which is expired; and consequently, in respiration, animals are consumers of carbon, and its consumption is attended with the production of animal heat. Vegetables, on the contrary, absorb or take in carbonic acid, and exhale oxygen by their leaves, and consequently are accumulators of carbon; and it may be, have the effect in evolving oxygen, of occasioning a reduction of temperature, or of creating a cooling process, the opposite of that of the animal-heating process. Should this be proved to be the ase, it will be another example of wise and most happy daptation.

I have spoken of vegetables, as organizers, or the producers of organic compounds for the support of animal life. Taking another view, animals may be considered as performing a part as essential to vegetable life, that of disorganizers; what is excrementious from them being so reduced as to have the character rather of inorganic than of organic compounds, whether it be carbonic acid with which they contaminate the air in respiration, their gaseous excrement, or their liquid and consistent, derived from the other excreting organs and passages of the body. These matters which are destructive to animals, and not only to the animals that void them, but to animals generally, may be held to be the highest kind and most appropriate food of plants. And the more we reflect on this, the more we are convinced of its truth—the more we must admire the connection and mutual dependence. The animal enriching the air for the use of the plant, the plant purifying the air for the use of the animal, and the same in regard to the soil, afford a lesson to man of a very instructive kind, most beneficial when carried practically into effect, most injurious when neglected; in the one instance insuring fertility, and I may add salubrity; in the other, the production of sterility and disease.

Let us now, for a moment, take a glance at the composition of plants and animals. Both may be considered as composed of nearly the same elements,—few in number, but variously united, so as to give rise to very many different compounds. The principal constituent elements of both are carbon, hydrogen, and azote, oxygen, lime, potash, silica, and phosphorus. Of these, carbon and silica preponderate in plants (silica, indeed, strictly is confined to plants); azote and phosphorus preponderate in animals. In plants, a large proportion of carbon and silica are expended in forming the woody fibre, the framework of the vegetable structure, and the epidermis, the resisting outer covering; whilst in animals, the azote and phosphorus are as largely expended in producing the organs of locomotion,—the muscles and bones. And in each instance we witness the usual happy economy of nature, and fitness of means to an end. Plants being fixed to the soil, take from it that which is almost always abundant in a fertile soil, *silica*, a substance, even in a thin and delicate layer, imparting great power of resistance, and far less soluble when acted on by rain, than the less common, or at least less abundant, phosphate of lime. Animals, on the contrary, being able to range abroad in quest of food, select such as contain phosphate of lime and azote, and in such compounds as admit of digestion and assimilation, and conversion into bone and muscle, following, in so doing, their natural tastes, undoubtedly instinctively directed.

Leaving these general views, it may be well to consider the subject we have entered upon somewhat in its details.

Physiologists who have directed their attention especially to the food of animals, have arrived at the conclusion, that, amidst the extraordinary variety of articles capable of supporting animal life, there are three which may be considered as of most importance, and, as it were, elementary alimentary substances—substances which are found in milk, viz., an albuminous matter, the curd; an oily matter, the cream; a saccharine matter, the sugar-of-milk. It seems to be proved by a wide induction of facts, that articles containing these substances, or their analogues, such as starch for sugar, muscle for curd, any kind of fat for cream, are fit for the food of animals generally, and that no articles are fit that do not contain more or less of these. These substances, taken into the stomach, are converted into a pultaceous semifluid chyme, from whence a milk-like chyle is formed, and from whence blood, by which every part of the body is nourished, in its constant circulation.

The results of the inquiries of physiologists, as regards the food of vegetables, have not been so well defined and satisfactory. As the sap of plants is a fluid, and transparent, we are sure that complete solution is essential as a preliminary, and that nothing enters the spongioles of the roots, organic in its structure—a state of perfect solution being incompatible with such structure. The principal part of the sap is water; in it are dissolved carbonic acid, phosphate of lime, carbonate of lime, carbonate of potash, and in very many instances silica. And these inorganic substances, I apprehend, are to the plant for its food what the organic substances before mentioned are to the animal for the same urpose; and these are not less elementary than those, as nutritive principles. The sap so impregnated passes from the rootlets by ascending vessels to the leaves, undergoing some change in its passage, but a greater change in the leaves, where carbonic acid gas is decomposed under the influence of light, oxygen evolved, and woody fibre either formed completely, or a substance formed about to become woody fibre, and to be deposited by the sap in its descent through another order of vessels. And as in the animal frame, very different compounds are secreted by different glands, so too, in the vegetable, a vast variety of compounds are produced by an analogous function of secretion; tubes and cells in the latter corresponding to glands in the former, the ultimate structure of which is also similar, the glands being congeries of tubes or cells.

Returning to the sap, it may be asked—and it is an important question—how are certain of the substances, which I have mentioned as essential to this nutritive fluid, dissolved in the water of the sap, such as phosphate of lime, carbonate of lime, silica,—themselves insoluble in water? My belief is, and it is founded upon experiments which I have made, that their solution is effected by the carbonic acid in the sap. It is well known how soluble carbonate of lime, and I may add carbonate of magnesia, is in water containing carbonic acid;—it is quite certain that phosphate of lime is also soluble in the same, and that not in an inconsiderable degree; and the experiments which I have made on silica, to me are convincing that it likewise is soluble in water impregnated with carbonic acid, though in a degree very much less than phosphate of lime.

Taking this for granted, a certain simplicity is imparted to the nutritive process of plants. A fluid medium, water, holding a gaseous acid, carbonic acid, is the menstruum of the inorganic substances derived from the soil, which the plant requires for its healthy growth. This compound solution becomes exposed, in the leaves, to the action of light, and to the evaporating agency of the winds; the carbonic acid undergoes decomposition, as already mentioned, carbon being detained for the use of the plant, oxygen being exhaled; a portion of the water is removed by evaporation, and, in consequence, the solvent power of the menstruum is diminished, and depositions of silica, and carbonate of lime, and other ingredients, take place. This view, it appears to me, is not only recommended by its simplicity, but also by a certain beauty and exactness of adjustment, and economy of means. Is it not very admirable that a gaseous acid, which, with water, is to yield to the plant, by decomposition, its organic elements, should be the solvent and vehicle of its inorganic parts?

Limited as I am in the delivery of this discourse by time, my main object has been to give general views, precise in themselves, and I believe correct, and involving principles capable of being carried out into practice,—the test and confirmation of scientific truths.

On the practical part of the subject of Manures, it is not my intention to enter at any length. I have neither time for it, nor the experience to do it justice, or to treat of it in the manner I could wish. In the farther observations which I propose to make, I shall restrict myself to such remarks as I hope may be suggestive,—may excite curiosity and inquiry, and so have a chance of being useful; intelligent inquiry being, as I think, the one thing perhaps the most needful,—without which agriculture can never speedily advance, and under which it cannot fail to advance, and from an art, which it is at present, and obscure in many of its parts, become a science, as certain as to results as is compatible with the uncertainty of the weather, and of other circumstances not under the controlling power of man.

What are the principal sources of manures, or of fertilizing means? I shall briefly speak of them under a few heads. 1*st*, Atmospheric, chiefly in the form of rain; 2*dly*, Of animal matter; 3*dly*, Of vegetable matter; and, *lastly*, Of mineral.

1*st*, Of the fertilizing means derived from the atmosphere. The atmosphere, as you are aware, is a mixture of two gases, ote and oxygen, in certain, almost constant proportions; d of carbonic acid and aqueous vapour,—the former in mall quantity, little variable,—the latter in a variable quantity, and extremely variable according to circumstances of temperature and other influences on which its increase and diminution depend. And, besides these ingredients, there is reason to infer, that various other substances are either suspended, floating as minute particles or dissolved in the air, such as carbonate of ammonia, common salt, and some other salts derived from the sea, and dust of several kinds. The most important of all these atmospheric ingredients are, undoubtedly, the oxygen, azote, carbonic acid, aqueous vapour, and ammoniacal salt. I should exceed my limits as to time, were I to enter into particulars on the fertilizing agency of either one of these substances. Aqueous vapour condensed and precipitated, you know, occasions rain,—is, in fact, rain. When it falls, it brings down with it carbonic acid, some oxygen and azote, and, there is reason to believe, a minute portion of carbonate of ammonia—all dissolved in it. Moistening dead vegetable and animal matter at the surface of the earth, it favours their decomposition, and the evolution of carbonic acid and of carbonate of ammonia; penetrating beneath the surface, descending into the soil, it has a like effect there; and there becoming impregnated by its solvent power with what is found in a fertile soil,—the substances already alluded to, as phosphate of lime, carbonate of lime and magnesia, carbonate of potash, and silica,—it passes into the growing plant, absorbed by its roots, and becomes its nutritive sap. Thus complicated is rain in its agency; so various are the circumstances which concur to this agency, and so happily are they connected, one favouring the action of the other, and all promoting the process of vegetation. Reflecting on these circumstances, we cease to wonder at the growth of forests in a state of nature, in which for a long series of years, vegetable matter, living or dead, is constantly accumulating, deriving its elements solely from the atmosphere and the soil, and by what it abstracts from the former greatly enriching the latter.

2dly. Of the fertilizing means derived from animal matter I have already alluded to the composition of animal matter, and how, as regards its ultimate elements, excepting that it contains no silica, it differs, compared with those of vegetables, rather in proportion than kind. Readily putrefying under the influence of oxygen and moisture;—readily giving up its elements, in the form of carbonic acid, carbonate of ammonia, phosphate of lime,—it is easy to conceive how it may promote vegetable growth, and especially the growth of those plants into the composition of which much azote and phosphate of lime enter,—as in the instance of all the corn-bearing grasses, and all the leguminous plants, and, in brief, all those which are highly nourishing as the food of animals. When I speak of animal matter, I speak of it almost without exception, every part of an animal—(excepting, indeed, fat, a substance which contains no azote or phosphate of lime)—being adapted by its composition to have a fertilizing effect, whether bone or hair, skin or muscle, whether blood or urine. This general fitness of animal matter for the purposes of manure is most deserving of being kept in mind, associated with the fact, that the animal matter does not act, except when undergoing decomposition; that is, it is not the blood that fertilizes applied to the soil, but the elements of the blood, and so of the urine and other excreta. I beg to call your attention to this, because it appears to me that economy is not observed in the West Indies in the use of animal manures. Large quantities of guano are imported at a great cost, and applied to your cane-fields, whilst the bones of all the cattle that are killed for the market, or die of disease, are neglected. I speak of guano and bones thus together, because guano contains a large proportion of the same ingredients as bone, viz., phosphate of lime, and so far they are adapted to act the same part. Besides phosphate of lime, guano contains ammoniacal salts. It is a matter, as you are no doubt aware, derived from the excreta of sea-fowl—their urine and dung, partially decomposed. Its nature should be remembered: it should be remembered that the excreta of other animals are hardly less valuable, and, without exception, and as before observed, these are the matters rejected by animals, and noxious to them, which seem by nature specially intended to be the food of plants. Though it little attracts common attention, there is not an animal in its wild state that does not promote vegetable life; the urinary secretion of the smallest insect differs but little from that of the sea fowl, the source of guano; and, in consequence, even when insects are destructive they may fertilize, so that sometimes it may be a problem whether the good effected by them in one way may not preponderate over the evil they occasion in the other. But it is not my wish to appear paradoxical; I mention these circumstances because I believe them to be deserving of attention, and fit to illustrate the nature of animal manures.

3dly, Of the fertilizing means derived from vegetable matter. That vegetable matter should be fit to be the food of plants, is most easy to conceive *a priori;* and that it is fit, is proved by the most extensive experience, and this, generally, and without exception, in different degrees, as in the instance of animal matter; and, also, as in the instance of animal matter when undergoing decomposition,—that is, when under the influence of moisture and atmospheric air, the vegetable matter is becoming resolved into the compounds from whence it originated. It is a happy circumstance for agriculture, that vegetable matter in a dry state undergoes little or no change on the surface of the soil; it also is a happy circumstance, that it undergoes little or no change under the soil, even when moist, if atmospheric air be excluded. Moisture and atmospheric air are essential to the growth of plants; and, being essential also to the decomposition of vegetable matter, the latter takes place when it can be useful to the former. One of the peculiarities of cane cultivation in Barbadoes, and, I apprehend, a very judicious one, is the strewing of the leaves of the last crop on the surface of the cane-field to protect the young plants from the sun's rays and from parching winds. Conjoined with this effect, is another which takes place, though I believe not contemplated, viz., that of manuring the young canes. So long as drought prevails, the old leaves are protectors, themselves unchanged; so soon as the rains set in, even if there be only a single shower, their decomposition commences, and their fertilizing influence is exerted.

An idea commonly prevails, that soils by long culture are worn out and exhausted. Under one system of cultivation this may be,—as when the crops, the produce of the soil, are in no part returned to it; but, it cannot be in ordinary course, if the reverse practice be observed; the tendency of vegetation being to enrich the soil: in brief, the great fertility of what are called virgin soils, appears to be owing, not to a state that the word implies, but to the plants which have grown on such a soil, having undergone decay there, and by their decay manured and enriched the soil. Take the example of a native forest:—the trees composing it spread their roots in all directions and to a considerable depth, from whence they collect the inorganic materials requisite; these are conveyed into the leaves and into the wood of the trunk and branches, in which also, carbon, derived from the atmosphere, is stored; the leaves fall and decay, and, in time, the trees, and decompose, and in decomposing give carbonaceous matter to the soil, and restore the inorganic materials extracted, and as it were, collected and concentrated. Such is the natural tendency of vegetation; and such also is the effect of judicious management, when green crops are ploughed-in as manure, or when the greater part of the ripe crop is returned to the soil, whether directly, as in the instance of the application mentioned of the leaves of the cane to the surface of the soil; or, indirectly in the state of stable-dung, or pen-manure, the vegetable matter having been first used as forage. This view is simple and clear, resting on the principle that vegetable growth and the enrichment of the soil are concomitant; and, I believe, that generally it is a principle of practical application. There are, however, facts deserving of attention, and of careful study, which prove that the growing of plants of the same kind for a series of years, the plants decaying where they grew, has an injurious effect on the soil, as regards its power of supporting these plants, indicated by their disappearing, followed by other species which grow luxuriantly, proving unexhausted fertility, that is, that the soil has been so modified by one species as to be rendered unfavourable to it, and yet favourable to another species. Now, what is witnessed in a striking manner as the result of a long series of years in the soils under forest growth, may take place, though not in a marked manner, from year to year in artificial culture; one plant, though not carried off the soil, may be injurious in preparation for a crop of another plant. I make this remark, keeping in mind a fact which was mentioned to me by a gentleman of Barbadoes, one of its ablest practical agriculturists; it was, that on some estates in this island, the growing of guinea corn, and the ploughing or turning it into the soil in its green state, had a decidedly injurious effect. This partial effect, and the effect before alluded to, are problems which are yet unsolved, doubtlessly admitting of solution, but of solution which can only be attained by scientific inquiry. In the instance first mentioned, the soil beyond a certain depth may be exhausted of the inorganic matter required by the plant, and which may be accumulated at the surface in a state unfavourable to the exhausting species, and yet favourable to another species. In the last mentioned instance, the guinea corn may so unite the inorganic elements which it extracts and returns to the soil, make such compounds of them, as to be unfavourable either to their solution and entering the sap, or, if soluble, to their affording supporting nourishment to the cane. These are mere conjectures; but whether true or false, can be determined only by exact research.

Lastly, Of the fertilizing means derived from mineral or inorganic matter. The most fertile soils appears to be those which are most compounded, which contain the largest number of the inorganic elements of plants, and in a state of minute division, favouring their solution to enter into the composition of the nutritive sap. Lime, magnesia, silica, potash, phosphate of lime, may be mentioned as the most important of these; and these may exist in the soil in different states,—either as free, uncombined, or in combination, constituting mineral species. If the former, they are more readily yielded up to the growing plants, and the soil without care is in danger of being sooner exhausted; if the latter, they are yielded up more slowly as the minerals decompose, and, in consequence, exhaustion, even with bad management, is difficult. Such fertilizing means as these in the soil, depending on its supplying the inorganic elements of

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plants, are obvious. There is another source of fertility in the soil of a more obscure kind. There are some soils, for example, capable of yielding nitre, and that for successive years. Nitre, you know, is a compound of nitric acid and potash. When formed in a soil, there is reason to infer that invariably the alkali is furnished by the soil, derived from a compound mineral, and that the elements of the acid, azote and oxygen, are furnished by the atmosphere, and that in consequence of certain ingredients in the soil, favouring the union of these elements, especially lime, the acid is formed and the salt is produced. So there are other conditions of soil in which clay, and perhaps oxide of iron, act a part, promoting the production from its elements, viz. azote and hydrogen, of ammonia, or the volatile alkali. And, as the substances first mentioned, lime, magnesia, potash, silica, are fertilizers as inorganic elements; these latter, nitre and the volatile alkali, are probably useful in administering to the formation of the organic parts. In taking a view of the capabilities of a soil, the attention should be given to the under portion, to the subsoil as well as the surface soil. If the latter be neglected and never turned up and brought into use, great may be the loss to the agriculturist. Water holding carbonic acid in solution, I have endeavoured to shew, is the principal agent or menstruum by which the sap is formed and plants are fed. This is the result when it is absorbed by the roots, and is transmitted by their ascending to be distributed to the various parts where required; but when, instead of being thus absorbed, it passes from the soil to the subsoil, it impoverishes the former and enriches the latter, removing out of it more or less of the soluble constituents, so that you may have at the same time an exhausted surface soil and a rich subsoil, requiring only a change of place, it may be, a mixing of the two, to become highly productive.

Gentlemen,—I must now bring this discourse to a conclusion. As I began it with drawing your attention to the analogy between plants and animals, I do not know how I can etter finish it than by reverting to that analogy. All I have itherto said, has been on the subject of manures generally, as involving common principles, universally applicable. Not a less important part of the subject, is that of special manures, or of the choice of manures for particular crops, and this is by far the most difficult part, and the one hitherto least studied. In animals, their coarser organization is distinct; if we examine the teeth of any particular animal, its stomach, its intestines, the main organs concerned in its nourishment, we have no difficulty in deciding from their structure, whether the individual is carnivorous or herbivorous, or fitted for, and requiring a mixed diet of animal and vegetable food; and this, be it one of the mammalia, birds, or insects. But not so as regards plants; the organs concerned in their nourishment are so minute as to escape detection by the eye unaided by the microscope; and even when examined by this help, no differences characteristic have hitherto been detected, admitting of being associated with the quality of the nourishment best fitted for the individual plants. This, then, taking it for granted, and it seems to be proved by experience, that different kinds of plants do not for their coming to perfection, require *one kind*, but *different kinds* of manure, must be determined by other means. How is this important object to be accomplished? I do not know how it can be well accomplished, except by enlightened experience and by chemical research. In the instances of the corn-bearing grasses, such experience, such inquiry have been highly useful. These are crops which are exhausting to the soil,—the grain being consumed at a distance, and the more exhausting when the straw also is removed from the farm. To correct the exhausting effect, one of two measures is adopted; either to allow the land to remain fallow for a certain period, during which, owing to the decomposition and disintegration of mineral particles in the soil, and the addition made to it from the atmosphere by the agency of the elements, and by rains and winds, and from the subsoil, by the penetrating roots of native grasses and other indigenous plants, the loss is made good of those inorganic materials carried away:—or, in a more summary manner, by restoring in manure (aided by intervening green crops), the ingredients abstracted and lost in the grain and straw; in the former, chiefly phosphate of lime and azote; in the latter, chiefly silica. The summary or shorter way, last mentioned, is the improved method, the result of scientific research; the tedious way, first noticed, is the result of mere experience unaided by such research, and is in many respects imperfect. The same remarks, I believe, are applicable to every kind of crop. Take the most important with which the interests of the West Indies are connected, the sugar cane. For its most successful cultivation, that is, its most profitable, as regards its produce and returns, supposing the fallow system to be put aside as least judicious, it needs to be determined what kind of manure is best fitted to bring the cane to its perfection of growth as a sugar bearing plant, and with most economy, keeping in view the balance sheet of an estate. If the problem is entered upon by the agricultural chemist, as I apprehend it should be, he will have to consider what are the elements constituting sugar; they are oxygen, hydrogen, and carbon, elements derived from the atmosphere, either directly or indirectly: he will have to consider what are the elements of the saccharine juice, as extracted by pressure from the plant; these, besides sugar, are, I believe, chiefly starch and albuminous matter, or a matter containing azote, and capable of exciting the vinous fermentation, and one or more vegetable acids: he will have to consider what are the elements of the cellular structure of the plant in which the saccharine juice is contained, and the composition of the other parts of the cane; these appear to be very much the same as the constituent elements of the stalk and leaves of the other grasses, viz.—phosphate of lime, silica, lime, magnesia, and potash, with the common elements, carbon, oxygen, and hydrogen. Having given consideration to all these matters, he may offer suggestions for trial. As the cane contains little azotized matter, and pure sugar none, he may suggest that the manure most likely to be successful is one composed chiefly of vegetable matter, or one in which the proportion of animal matter is small. As the sugar and other exports, viz. rum and molasses, contain very little of any matter of a mineral kind derived from the soil, he will conclude, that with good management, a cane-crop is not exhausting; and that if the leaves and stalks are returned to the soil, the land instead of being impoverished, may be actually enriched, in as much as there is given to the soil a vast accession of vegetable matter, the elements of which have been obtained from the atmosphere, and which, in decomposing, render up these elements to support the growing crop, being, as it were, to the young plant what the milk of the mother is to the young animal. I should add, that the inquiry of the agricultural chemist will by no means be complete, unless his attention also be directed to the soil, and that not only once, but at intervals. If, on his first examination of it, he find a marked deficiency in it of phosphate of lime, and of other inorganic substances which seem to be essential to the composition of the sugar cane in its healthy state, he will suggest the use of guano as a manure, or of bone-dust, or of marl containing phosphate of lime, according to circumstances. If, after an interval of a few years, the land having yielded good crops, he examine the soil again, and now finds in it no marked deficiency of phosphate or carbonate of lime, or other supposed requisite inorganic ingredient, he will, I conceive, be warranted in suggesting the sparing the expensive manures, the guano and the bone-dust, and using only manure chiefly vevetable, made on the estate.

Such, I fancy, is the line of inquiry, as regards particular manures, that is likely to be most useful, and the more useful, I cannot but think it will be, the more minutely, and carefully, and judiciously it is carried out, testing theoretical suggestions founded on chemical analysis, by the results of practical experince, that is, by well conducted trials, and extending them from point to point, till satisfactory knowledge is arrived at, so that being acquainted with the quality of the soil, the quality and quantity of manure applied, the mode of tillage employed, the planter may be able to calculate, *communibus annis*, what will be the quantity and quality of sugar, what the quantity and quality of the molasses, and what the quantity of rum, that should be made on his estate.

The subject of manures is far too large a one for a single discourse: many parts of it I have treated of very slightly; some, I am sure, very imperfectly, and perhaps obscurely; your knowledge and intelligence will supply many deficiencies, and I am confident I may equally rely on your kindness for exercising them, recollecting (be pleased to do so) that my main object has not been so much to impart minute information, as to inculcate some general principles, and to endeavour to excite inquiry, that methodical and scientific inquiry which leads to exact knowledge, and to the perfecting of, when diligently carried out, every art to which it is applied.

On the Foundation of a New Geology. By Professor G. Bischof of Bonn.

"For a number of years," says Professor Bischof, "I have contemplated the publication of a geology based on chemical and physical principles. With this view, I have made observations during journeys, principally in the neighbourhood of my own residence, which is so interesting in a geological point of view, and I have carried on experiments in my laboratory, in order to study nature in her formations and changes. It will readily be admitted that it was necessary to examine geology in a chemical and physical point of view; for our geologists, for the most part, are neither chemists nor natural philosophers; and to endeavour to explain, without chemical or physical knowledge, the chemical operations which have taken place in the great laboratory of nature, appears to me a vain attempt. Werner wished to explain all by the Neptunian theory, at a time when chemistry had scarcely done anything for geology. His system is exploded. It has been succeeded by Huttonianism, which has likewise been pushed too far, and has done more injury in its turn than all the Wernerians. I shall shew that it is altogether a vain attempt to endeavour to explain all by either of these two methods.

In the present state of science, we cannot doubt that the crystalline rocks owe their origin to an igneous fusion. But we no longer meet with granite, basalt, diorite, &c., in their primitive condition; water has everywhere produced changes more or less considerable, and we possess many observations on the porosity of the densest rock. The whole family of zeolites, all the hydrated minerals, and the carbonates in the crystalline rocks, are secondary formations of minerals originally the same, but now wrought up again by carbonic acid, oxygen, and the other bodies which form a part of the water. Mineral springs which issue from great depths only form a particular case, and these are the waters which, with a small quantity of carbonic acid and oxygen, have produced at the surface the most important phenomena, and, as I shall shew, are producing them still.

During my investigations, I have found great assistance in the great number of facts which mineralogical chemists have made known; but there are, at the same time, many blanks to be filled up, and my own exertions will be insufficient for that purpose, unless I obtain assistance.

It may be demonstrated mathematically, that all the sedimentary formations, transition masses (with the exception of transition granites and basalts), and all the substances inclosed in drusy cavities, are derived from crystalline rocks. These have furnished the materials, and the waters have conveyed the quartz, calcareous spars, heavy spars, metals, and other substances which fill the cavities, into their present beds. Of this I am perfectly convinced by an examination of the amygdaloidal rocks of Oberstein.

M. Rose has informed me, that every time he has made sulphuretted hydrogen pass into minerals dissolved in acids, he has remarked particular reactions. The precipitates have often presented copper, and as this precipitate is always very small, and does not enter into the formula, it has been neglected, as forming an uninteresting mixture. In one point of view, however, these minimum quantities are of very great importance, and it is to be regretted that the anxiety to find a chemical formula should be greater than that which should be manifested in searching out mixtures continually intervening, although in the very smallest proportions. For example, if it could be demonstrated that in the species of felspar, as in the Amazon stone, copper and other metals are always present, although in the smallest proportions, nothing could be more easy than to explain the origin of minerals in rents and veins, by taking into account the enormous masses of felspar which must have been destroyed by atmospheric phenomena. The minimum quantities in fossils usually indicate the way in which the fossil has undergone a transformation. Thus, the small proportion of potassium in fahlunite, indicates its passage into mica, and the mineralogical researches of M. Haidinger strengthen this opinion. How important, then, would be the comparative analyses of condiorite, fahlunite, gigantolite, pinite, &c., if we met with them simultaneously in the same crystalline rocks, and passing into one another!

When we observe barytes as the distinct companion of the mineral manganese, we cannot doubt that the manganese has been conveyed from mountain-rocks into transition-fissures; consequently, whenever we meet with oxide of manganese, we ought also to find barytes. We are also forced to admit that the barytes must be present, in the state of silicate, in the amygdaloidal rocks, when we find it in the anfractuosities of barytic hermatose. In the amygdaloidal rocks of Oberstein, in the anfractuosities of which this species of hermatose occurs, I have not hitherto, it is true, met with barytes, but very obvious traces of strontian are perceptible. Certainly barytes often presents itself to chemists in the analysis of minerals; it may often, in consequence of its small proportion, have been confounded with lime, and the chemist who follows a formula, pays little attention to it if it contradicts the results. But although I express myself with some freedom on the disorder of formulas, I think we cannot dispute their competency to the author of a stochiometry. When chemical formulas are used with reserve, they may be of great advantage; but it is truly puerile to see the efforts that are made to include, for example, all the results of the analyses of the tourmalines in a single formula.—*L'Institut.*

On the Relations of Ethnology to other Branches of Knowledge.

By J. C. PRICHARD, M.D., F.R.S., Corresponding Member of the Institute of France, &c. &c. &c. Communicated by the Ethnological Society.*

The anniversary address delivered at the last meeting of this Society by our late excellent President Sir Charles Malcolm, gave so lucid and extensive a survey of the recent progress of Ethnology, that I should find little to offer on the present occasion, were I to follow the same path. The achievements of one year, taken by themselves, would appear fragmentary, and without results. It has, however, occurred to me, that there is a different course by which I may hope to fulfil the task allotted to me more to the satisfaction of the Society. The idea has been suggested to me by a proposal made lately to the British Association for the Advancement of Science, to appoint in that Society a distinct Section for the Cultivation of Ethnology.† In some parts of the Continent, and in the United States of America, societies have for some time existed exclusively devoted to this pursuit, and supported by men highly distinguished in science and literature. In the meetings of the British Association alone, Ethnology claims but a subordinate place in the Section of Natural History. The reason assigned for this arrangement is, that the natural history of man is a part of the natural history of living creatures, and that there is an obvious propriety in referring to one division, the history of all organised beings, namely, of all those beings which exist in successive generations, destined one after another to rise, flourish, and decay—a lot to which are alike subjected the lords of the creation, and the worms on which they tread—and the plants and animals which they consume for their daily food. But though the natural history of man may, in a technical arrangement, be made a department of zoology, it is easy to shew that the main purport of ethnological inquiries is one distinct from zoology; and the reference of both these subjects to one section of the British Association can only have arisen from inadvertence. Of this I shall be able to convince the members of the Society now present, if they will allow me to call their attention for a brief space to the position which Ethnology ought to hold, and which it is destined hereafter to maintain, among the various divisions of human knowledge.

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* Read before the Ethnological Society, June 1847.

† This proposal was made at the meeting of the Association at York by Dr King, secretary of the Ethnological and Statistical Societies. It was negatived by the Committee of the Association.

Ethnology is the history of human races, or of the various tribes of men who constitute the population of the world. It comprehends all that can be learned as to their origin and relations to each other. It is distinct from natural history, inasmuch as the object of its investigations is not *what is*, but *what has been*. Natural history is an account of the phenomena which Nature at present displays. It relates to processes ever going on, and to effects repeated, and to be repeated, so long as the powers of Nature, or the properties of material agents, remain unchanged. Ethnology refers to the past. It traces the history of human families from the most remote times that are within the reach of investigation, inquires into their mutual relations, and endeavours to arrive at conclusions, either probable or certain, as to the question of their affinity or diversity of origin. All this rather belongs to archæology than to natural history. It may, indeed, be truly said that the investigations, by means of which we endeavour to arrive at conclusions in Ethnology, involve many topics which are within the province of natural history. The facts and analogies which natural history presents, are the data on which a great part of the proofs or arguments adopted by the ethnologist are founded. But these contributions of natural history are only a part of the resources by the aid of which we carry on the investigations belonging to our favourite pursuit; and we shall find that it borrows fully as much from other departments of knowledge, quite separate from the study of Nature and her productions. The results at which the ethnologist arrives, do not fall within any department of natural history. They are archæogical or historical. It may then be admitted, that there e some grounds for the opinion of those who would even ny us any place in the great system of scientific inquiries, which the British Association has established,

We are saved from apprehension as to the consequence of this admission, by remarking, that Ethnology stands exactly on the same ground, in this point of view, as one of the most popular of the studies which are cultivated at the British Association; and that it is impossible, with any shew of reason, to deny a place to one of these sciences in the arrangement of sections, without refusing the claims of the other. By comparing the position of ethnology to that of geology, we shall be enabled to survey, in a clear point of view, the relations of each of these sciences to other branches of knowledge.

Geology, as every one knows, is not an account of what Nature produces in the present day, but of what it has long ago produced. It is an investigation of the changes which the surface of our planet has undergone in ages long since past. The facts on which the inferences of geology are founded, are collected from various parts of natural history. The student of geology inquires into the processes of Nature which are at present going on, but this is for the purpose of applying the knowledge so acquired to an investigation of what happened in past times, and of tracing, in the different layers of the earth's crust—displaying, as they do, relics of various forms of organic life—the series of the repeated creations which have taken place. This investigation evidently belongs to *History* or *Archæology*, rather than to what is generally termed *Natural History*. By a learned writer, whose name will ever be connected with the annals of the British Association,* the term Palæontology has been aptly applied to sciences of this department, for which Physical Archæology may be used as a synonym. Palæontology includes both Geology and Ethnology. Geology is the archæology of the globe—ethnology that of its human inhabitants. Both of these sciences derive the data on which they found conclusions from the different departments of natural history. But ethnology likewise obtains resources for pursuing the investigation of the history of nations and of mankind from many other quarters. It derives information from the works of ancient historians, and still more extensively from the history of languages and their affiliations. The history of languages, indeed, greatly extended as it has been in late times, has furnished unexpected resources to ethnology, which could hardly have advanced a few steps without such aid. As geology would have been a barren and uninteresting study, and uncertain in most of its results, without the aids which the study of organic remains has unexpectedly brought, serving often to identify geological formations, and to connect particular series of rocks with periods in the world's history; so the discoveries of Glossology have enabled us to trace alliances between nations scattered over distant regions of the earth, of whose relation to each other we never should have had an idea without such evidence.

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* *The Rev. Dr* Whewell.

I shall now endeavour to point out, in a brief and cursory manner, what assistance each department of knowledge has contributed to the cultivation of ethnology. But here I must crave the indulgence of those who are conversant with the history and progress of this science, since I shall not be able to accomplish the task proposed without saying much that must to such persons be already well known and familiar.

I. The branches of natural history and science which furnish aids in the promotion of ethnology, are anatomy, physiology, zoology, and physical geography.

The first attempt that deserves notice to distinguish races of men was made by Camper, an anatomist. The distinction adopted by Camper is well known. It is founded on the shape of the skull. It is the facial angle, or the measurement of the angle included between two lines, one of which is drawn from the passage of the ear to the basis of the nose; and the other a line slanting off from the forehead to the mouth, or rather to the most advanced point of the upper jawbone. This angle was thought to afford a measure of the capacity of the anterior part of the skull, and of the size of the corresponding portion of the brain. Camper, who had within his reach very few skulls for examination, thought that he found this angle of different extent in different classes of human heads. He found that skulls of Europeans, when thus measured, gave an angle of 80°, the skull of a Kalmuck one of 75°, and the skull of a Negro one of 70° only. There are forms of the head in which the angle has been found to be greater than in the European, and others in which it is less than in the Negro. Those which have it greater than in the European, and in which it amounts to 90°, are the ideal heads of Grecian gods, forms not existing in nature; and the skulls in which this angle is less than in the Negro, are those of apes. In these last, the angle was estimated by Camper at 64°, 63°, or 60°. Camper accordingly thought that he found in the skulls of negroes a type intermediate between the cranium of the European man and that of the Orang. But in this he was mistaken. The supposed gradation exists only when skulls are compared which have the infantine form, or before the first dentition is complete. After the period of the first dentition, the difference in the facial angle in the heads of apes, and in those human skulls in which it is of the smallest measurement, becomes enormous. In the adult Troglodyte it is 35°, and in the orang or satyr it is only 30°, as we learn from the observations of Professor Owen.

Professor Blumenbach was, in reality, the founder of ethnology. He was the first person who made any considerable collection of human skulls, or possessed the materials requisite for an inquiry into the anatomical differences which exist in various tribes of men. Blumenbach divided the forms of the human head into five departments. He designated them, not as it would perhaps have been better to have done in the first instance, by epithets descriptive of forms, but by the names of the races of people to which they belonged, or of the regions of the world whence these races were supposed to have originated. The Caucasian form was so termed from Mount Caucasus, to which Blumenbach observed that ancient traditions refer the origin of many celebrated nations. He supposed this to be the primitive type of the human skull, and regarded the other forms as so many degenerations from it. These were the Mongolian, the American, the Ethiopian, and the Malayan. The five forms were supposed to belong to five divisions of mankind, comprising collectively the whole human family. This distribution wa complete as far as the ethnographical knowledge of th time allowed it to be ; but it would be necessary in the presen day to enumerate many additional varieties in the shape o the skull, and to constitute additional human races, if w would follow the same method, and adapt it to the actua state of our acquaintance with distant regions of the eart and their inhabitants. For example, besides the Ethiopia race of Blumenbach, by which he meant the Negroes, we must reckon in Africa two other woolly-haired races, each having a form of the head different from the Ethiopian type of Blumenbach. I allude to the Kafir and Hottentot races. Again, among the nations termed collectively by Blumenbach the Malayan race, meaning the native people of all the islands of the Great Southern Ocean, we now distinguish several different forms which have little or nothing in common, and appear to belong to several distinct races. Among these are the Papuas, who resemble the Negroes in many respects, but have skulls of a form very different from any of the African nations, and the Australians having a peculiar type, and forming a very distinct race. The Polynesians are not so distinct in the form of their heads as Blumenbach supposed the Malayan race to be ; and the true Malays ap-

The latest scheme devised for the classification of human kulls, and the distribution of human races according to their orms, is that proposed by Professor Retzius of Stockholm, a very ingenious and able anatomist, and a very estimable man, who has lately devoted his talents to this subject. Professor Retzius' researches are well known, and I shall not occupy the time of the Society by stating to them his results. They are particularly interesting in one point of view, probably not contemplated by the excellent author. I allude to the fact, that he seems to have established distinctions in the form of the skull among nations, who, though for many ages separate, are known, if I am not mistaken, historically, to have descended from the same original stock.

The head is not the only part of the body which displays different forms in different human tribes. Varieties in stature and in the proportion of limbs—in the form of the pelvis and other parts of the body—as well as in the texture of the skin, the hair, and other structures, are well known to distinguish races from each other. With respect to all these differences, anatomical researches have been made which have an obvious bearing on ethnology.

When we advert to the resources which physiology affords for the cultivation of ethnological science, we find that there are many relations between these studies. One series of inquiries is, whether the great laws of the animal economy are the same in respect to all human races; whether any particular race differs from others in regard to the duration of life, and the different periodical changes of constitution, and, generally speaking, in the laws of the animal economy, and whether such diversities, if found to exist, can be explained by reference to external causes, or imply original difference, and form, therefore, specific characters. Another physiological inquiry connected with ethnology is, whether variations of form, colour, &c., can be explained by reference to any known principle, and how far, and under what conditions, they are transmitted to posterity, and may tend to account for the origination of particular breeds or tribes marked by some hereditary and permanent characters?

Zoology and the whole study of natural history opens a field to several inquiries highly interesting to the ethno gist.

We must take an account, for example, of the varieties form and organization to which the different species of a mals are subject, in order to solve the question, Wheth the differences observed in human races, and the physi characters of any particular race, belong to the category o natural varieties, or indicate an entire distinction which mu have existed from the creation, and therefore proves the spe cies to be distinct?

Another question connected with zoology has been made a matter of great moment in these investigations. I allude to the theory of Hybridity, or to the general observations made with respect to mixed breeds and their supposed sterility. The bearing of these questions on the physical history of mankind is very obvious. I shall not enlarge upon it at present.

Physical geography has a very obvious bearing on ethno logy, since physical geography comprises the localization of those agencies of climate which are supposed to modify the organization of living tribes, and which oppose limits to the sphere of their existence. We may observe, in connection with this subject, that great numbers of species, both animal and vegetable, are only to be found within certain latitudes, and in particular regions of temperature, and under particular local conditions. Those tribes in the animal kingdom which approach the human form, viz., the apes, are confined by nature to very narrow limits, while men live in all climates. This striking difference suggests several inquiries as to its causes, which have a bearing on physical geography.

One series of facts connected with physical geography, and having a bearing on ethnology, comprises the effects produced upon climate by elevation of the surface of the earth. It is well known that, in the ascent of mountains, changes of climate and of vegetation are perceived, analogous to those observed in passing from a lower to a higher latitude. Plants of the frigid zone reappear in equatorial countries near the summits of Alpine mountains. An inquiry is suggested, how far this may tend to explain the phenomenon, that the xanthous, or very fair complexion, with red hair and blue eyes, is often seen in the inhabitants of alpine regions. For example, if we begin from the eastern parts of the world, we find xanthous people on the Himalaya, in the Hindu families who reside near the sources of the sacred rivers at Jumnotri and Gangotri. Even the Rajpoots in Rajast'han, are known to be much fairer than the people of lower plains in Bengal and elsewhere. Passing the Indus, we find the Eusofzyes in the high tracts of Afghanistan, a fair xanthous people, while the Ghiljis of the lower country are dark, and the Jauts of the plain of the Indus nearly black. Then, again, we find on the high mountains of Hindu-Khu that curious people the Siah Posh, who speak a dialect allied to the Sanskrit, and are supposed by Bopp and Ritter, and other learned men, to be descendants from the ancient Brahman race who conquered India some thousands of years ago, and probably left these people behind them in Central Asia. Far westward in Arabia, according to Bruce, a similar phenomenon is displayed among the inhabitants of the cold mountains of Raddhua; and in Africa, the Kabyles, the natives of Mount Aurasius, behind Tunis and Algiers, are so fair, and red-haired, that they have been conjectured, without a shadow of proof, to be descendants of the ancient Vandals, as if it were possible for Vandals to have made their way to the tops of all the mountains in the world. Even in America, among the Ioways, and other tribes inhabiting the Rocky Mountains, similar facts have been observed. The rationale of this phenomenon is plainly connected with the physical conditions of so many mountainous regions. It admits a comparison with changes of complexion, discovered as we proceed from the country of black races under the equator, to that of the fair people of Northern Europe.

Another observation to which I shall briefly advert, will serve to indicate the bearing which researches in physical geography may possibly have upon the studies of the ethnologist. The phenomena of vegetation probably indicate conditions of climate which are inappreciable by thermometers, hygrometers, and all our instruments; and when we advert to the fact *illustrated* by that great botanist Mr Robert

Brown, that the equatorial distribution of the great families of plants is extended to a considerable degree through the Southern Hemisphere, or that the Austral regions approximate much more to the equatorial in this leading character of vegetation than the northern climates of corresponding latitude, we are at liberty to infer, that the sum-total of those agencies of climate which affect organized beings is much more similar to that which exists under the equinoctial line in these countries than in the northern latitudes. I shall merely mention this conclusion in connection with the well-known fact, that the varieties of the human race which exist in the great southern continents, have much greater resemblance to the tribes who are natives of the torrid zone, than any of the aboriginal people inhabiting the northern parts of the world.

I have now gone over the branches of natural science which principally give assistance to students of ethnology, and it remains for me to make some remarks on the resources which history and archæology may be expected to furnish in aid of the same inquiries.

When we speak of historical inquiries as contributing to the promotion of ethnology, the meaning of the expression is not limited to information to be collected from historians. Much, indeed, is to be found in the works of such writers as Herodotus, Aristotle, Diodorus, Cæsar, Pliny, Strabo, and Tacitus, that is useful for investigating the history of those nations in Europe and Asia, which came within the knowledge of the Greeks and Romans; but all such information would be not only confined, but disjointed and fragmentary, without some more comprehensive method of investigation that may serve to bring the notices scattered through ancient writers, into a distinct and evident connection with the history of people actually known to us. Historical researches that may be applicable to ethnology, must occupy a wide field. They must collect all the different lights that can be brought to bear on the history of nations, whether from the testimony of ancient writers, or from manners, customs, and institutions—from old popular traditions, poetry, mythology—from the remains of ancient art, such as architecture, sculpture inscriptions—and from sepulchral relics discovered in many countries, consisting of embalmed bodies, or more often the mere skulls and skeletons of the ancient inhabitants, which furnish the most authentic testimony, where it can be procured, as to the physical characters of various races of people. Besides all these, there is another source of information more extensively available than either of them; I allude to the history of languages and their affinities.

The history of mankind is not destined, like the facts on which geology is built, to be dug out of the bowels of the earth, though some of the ancients thought otherwise, if we may judge from the abundance of sculptures and inscriptions with which they covered the sides of caverns and excavations. Curious documents have, however, occasionally been discovered in various countries beneath the soil, which have brought evidence of historical facts otherwise unknown. We may allude, for example, to the great collections of silver money of the coinage of the early caliphs of Bagdad, which have been dug up in various places on the shores of the Baltic, marking out the path of an extensive traffic between the East and North, at a time when the northern people of Europe are generally supposed to have been in a state of extreme barbarism. But the discoveries most interesting in relation to ethnology are those of sepulchral remains, which, in various regions of the world, have preserved the most authentic records of the physical characters and the state of arts that belonged to many ancient races. I need hardly allude to the discoveries in the Egyptian Thebaid,—a vast sepulchre, where the successive generations of thirty centuries lie embalmed beneath their dry preserving soil, expecting vainly the fatal time, now long since passed, when they were to be summoned before the tribunal of Sarapis. Another African race exists only in mummies. I allude to the insular Guanches, the ancient inhabitants of the Fortunate Islands, who now, falsifying this name, exist only in the caverns of Teneriffe, or in the European museums to which they have been transported. Over vast wildernesses in the northern regions of Asia, along the banks of the Irtish, and beyond the remote Jenisei, innumerable tumuli are scattered, containing the remains of ancient, and, perhaps, long extinct races of men; and it is a remarkable fact, that in this wintry region where living nature seems to struggle against the elements for a precarious existence, even the arts of decoration were studied in those times of yore, which witnessed the erection of these tombs. Implements of silver, gold, and copper, girdles of the precious metals, bracelets decked with pearls, and fragments of porcelain, have surprised the travellers who have seen a few of these tumuli excavated. Whole nations lie perhaps yet buried in these regions, and with them may be found some relics that may hereafter throw light upon their history. Similar tumuli spread over the north of Europe, contain the remains either of the same people, or of races more barbarous than the Asiatics. Hundreds of them have been rifled by treasure-hunters, or by mere antiquaries little more enlightened, who have sought to make collections of curiosities without any view to promote ethnology or history. Of late years, Eschricht, Nilsson, and Retzius, have attempted, in Denmark and Sweden, to identify in these remains the relics of different races, supposed to have inhabited the northern region of Europe in early times. Their example has been followed by Dr Wilde in Ireland, and more recently by MM. Eugêne Robert, and Serres in France. It is too early to collect general results from these researches. I shall only observe, that, in the opinion of the learned Swedes who have devoted themselves to the investigation, the sepulchral remains of northern Europe belong to three different eras. They display three different physical types, and three successive stages of advancement in art and civilization. The oldest are the relics of a people with round heads, having the transverse diameter of the cranium large in proportion to the longitudinal. The implements and ornaments which are found in the tombs of these people, indicate the greatest rudeness of art. They consist of tools and the heads of lances and arrows made of stone and bone, but nothing indicating a knowledge of the use of metals. Whether these oldest sepulchres were the tombs of a Celtic race, is a question not yet decided. It seems to be the opinion of Professor Retzius, and of Nilsson, who has written a learned work on the Aborigines of

Scandinavia, that they were the burial-places of a people more ancient than the Celts. Similar remains discovered in France, are supposed by MM. Robert and Serres to have belonged to the Cymrian or Welsh branch of the Celtic race; and these anatomists suppose a second class of heads of a larger shape, found in tombs containing metallic implements, to have been those of a people allied to the Irish or Gaelic branch. A third set of monumental relics are referred by Retzius to a superior race, supposed to have been Swedes or Saxons, or some branch of the Teutonic family.

It is much to be regretted that the ancient nations of Europe, those races from whom Englishmen, Germans, and Frenchmen are descended, were so obstinate in their barbarism, that they despised the use of letters, and remained for centuries in intercourse with the cultivated Massilians, and with Roman colonies, without adopting this art; and that all the sepulchral remains of the northern regions are without inscriptions, or a single name that may be a clue to their various history. On the other hand, parts of Asia and Africa, now the seat of barbarism, are covered, if we may use the expression, with inscriptions. Numerous and long inscriptions scattered over all India on rocks, the sides of caves, and on various monuments, in Cabul, through the ancient empires of Iran and Assyria, through Hadramaut and Oman, the remotest districts of Arabia, and through the North of Africa, to say nothing of the more celebrated relics of Egypt, prove that the use of letters was well known in these countries at a time when Europe was barbarous. In all those countries inscriptions, which have been gazed at with stupid wonder by the descendants of the people who composed them, and have been regarded as the workmanship of genii and imps, have been at length read and explained for the first time after twenty centuries. All this has been done within a few years. The discovery began, as every one knows, with the deciphering of the Egyptian hieroglyphics. The efforts of Dr Young and Champollion gained the clue, unravelling mysteries in a field where it has been reserved for a distinguished scholar of the present day (the Chevalier Bunsen) to erect the edifice of the most ancient history of the world,—a monument of the intelligence of modern Europe more exalted than the royal pomp of the Pyramids, whose real builders now, for the first time, come forth to our view after having been concealed in the rubbish of 4000 years. Scarcely less remarkable is the achievement of our illustrious countryman Mr Prinsep, in the East, who has read and interpreted the inscriptions spread over India and Afghanistan. It is a curious fact, that these most ancient records of the furthest East preserve not the victories of warriors, but the decrees of Buddhistical sovereigns, commanding throughout the provinces of their great empire the establishment of hospitals for the cure of men and brute animals. Many curious facts in history have been preserved by these inscriptions, and among others the extension of a Macedonian empire over a great part of India, and the conquest of the Island of Ceylon by a sovereign of Hindustan three centuries before the Christian era. Not less remarkable are the inscriptions cut in letters composed of wedge-shaped strokes which are spread through the empire of the great Cyrus, and have been lately read. These were engraved by the subjects of the Persian kings. Another set of these cuneiform inscriptions belonged to the older Assyrians and Babylonians. The clue to all these discoveries was obtained by Dr Grotefend, Lassen, and Burnouf; and by its aid our countryman, Major Rawlinson, has succeeded in reading the history of the Achæmenidæ engraven on their own monuments in a language which was doubtless spoken at the courts of Susa and Persepolis, but has not been heard since the overthrow of the last Darius. Even the old Assyrian inscriptions are now partially understood, and the name of Nebuchadnezzar has been found on the walls of his palaces.

Many ethnological facts may be collected from these inscriptions. I shall instance the supposed existence of the Affghans among the nations subject to Darius, and who, doubtless, contributed to form the armies that fought at Marathon and Thermopylæ. It would be curious to find the ancestors of Akhbar Khan among the invaders of Europe 2000 years ago.

The inscriptions spread through Arabia and Ethiopia will probably throw light on the most ancient relations between Asia and Africa. We may expect to find in them the history of those queens of Ethiopia who reigned successively under the name of Candace, known to the generals of Augustus Cæsar, and one of whom is mentioned by St Luke the Evangelist.

I shall only refer to another set of inscriptions deciphered within a few years in several of the ancient Italic languages, by means of which we have gained some knowledge of the languages spoken in Italy before the ascendancy of Rome. They have afforded an ethnological result, which is also of some importance in relation to classical history. It seems from them that the old Italic nations, the Latins, the Umbrians, the Opici or Oscans, the Ausonians, the Siculians, the Samnites and Sabines, all the old Italic nations except the Tuscans, were not, as the older writers, Frêret, Larcher, and even as Niebuhr supposed, partly Celtic or other barbaric tribes, and partly Greeks, or at least Pelasgi, but a distinct and particular branch of the Indo-European family of nations, and that they all spoke dialects of one language, which may be termed the old Italic, and of which Latin is but one variety.

The most important aids to historical researches into the origin and affinity of nations is undoubtedly the analytical comparison of languages. This may be considered as almost a new department of knowledge, since, although long ago sketched out, and followed to a certain extent, it has been wonderfully augmented in recent times, and it is only in its later development that it comes to have any important relations with ethnology. Leibnitz is considered to have been its originator. The Adelungs, Vater, Klaproth, Bopp, Frederick Schlegel, and Jacob Grimm, have been among its most successful cultivators ; and lastly, to William von Humboldt it owes its greatest extension and the character of a profound philosophical investigation. But it is not, in this point of view that I contemplate the results of philological researches. It is as an auxiliary to history, and as serving in many instances to extend, combine, and confirm historical evidence, that the comparison of languages contributes to the advancement of ethnology. reat caution is, however, requisite when we attempt to draw inferences as to the relationship of nations from the resemblance or even identity of their language. We know that conquests, followed by permanent subjugation, have caused nations to lose their original languages and adopt those of their conquerors. The intercourse of traffic between neighbouring countries, the introduction of a new religion or of new habits of life, especially when rude and barbarous tribes have been brought into near connection with civilized ones, have given rise to great changes in the original idioms of nations, and have caused languages originally different to approximate. It is only when we have good reasons for believing that no contingent event has interfered to change the original speech of any particular race, or supplant it by the idiom of a different tribe, that we can be justified in founding on such ground an argument as to affinity in descent. Evidence may be collected on this point sometimes from historical facts, or from considerations founded on the known condition of particular nations. When we learn from history that two nations have been remotely separated from each other from a very distant age, and have never been brought into habits of intercourse, we may presume, that marks of affinity discovered in their languages can bear no other explanation than that of an original unity of descent. In other instances, phenomena are discoverable in languages themselves which enable us to determine whether traits of resemblance have been the effect of late intercourse between nations, or arose in the original development of their languages, and thus prove a common origin in the tribes of people who speak them. A careful analysis will often detect analogies of such kind as to afford undoubted evidence of primitive affinity between languages which have acquired in the lapse of time and the course of events great differences, and when each dialect has become unintelligible to people who speak another of the same stock. The investigation of affinity between languages has lately assumed the character of a scientific study, and when pursued with reference to certain general principles, has led to striking and important results. I shall briefly advert to some of these principles which have not yet been stated, as far as I know, in a systematic manner.

It is the prevalent opinion of philologists, that the most extensive relations between languages and those which are the least liable to be effaced by time and foreign intercourse, are the fundamental principles of construction. Grammatical construction, or the laws which govern the relations of words in sentences, appear to be very enduring and constant, since it extends to whole classes of languages which have few words in common, though it is supposed that they originally had more. But beyond this, there is a cognate character in words themselves which pervades the entire vocabulary of a whole family of languages, the words being formed in the same manner, and according to some artificial rule. This may be exemplified by the monosyllabic structure of the Chinese and Indo-Chinese languages, and by the principle of vocalic harmony pervading the languages of High Asia, to which I shall have occasion again to advert. Of grammatical analogy, or correspondence in the laws of inflection and construction, we have a specimen in the Aboriginal languages of the New World, whose structure is known to be very complicated and artificial, and at the same time common to all the idioms of America which have been examined.

Another example of a more definite character is afforded by the grammatical structure of the languages of High Asia and Great Tartary, and a still more striking one by that of the Indo-European idioms.

Connected with the subject of the formation of words is the remark, that in the various branches of particular families of languages which spring by gradual development from the same root, the elements of words, consonants and occasionally vowels, are found to undergo changes according to certain rules. Particular classes of consonants in one language are substituted for other classes in another language of the same family. One European idiom, for example, substitutes palatine letters for sibilunts ; another rejects them both, and substitutes labials in their place. When corresponding phenomena can be traced through a great part of the vocabulary of two languages, we recognise a proof that the languages so related must have been derived from one root, the ramifications of which have been differently developed.

The existence of similar words in several languages, even when such resemblances are very numerous, does not, in all cases, afford proof that the languages in question belong to the same family, since intercourse between different nations often gives rise to the adoption of expressions by one tribe for the language of another, as the English have adopted a great many words from the French, and the Welsh a still greater number from the English. The question, whether a considerable number of common or similar words in two languages affords evidence of original connection between them, may be solved by adverting to the particular sorts of words which are found to resemble. Even when one nation has derived from another a considerable proportion of its entire stock of words, there often, and indeed generally, remains an indigenous or aboriginal vocabulary, if I may be allowed the expression, or a home-bred speech, consisting of such words as children learn in early infancy, and in the first development of their faculties. This domestic vocabulary consists of words of the first necessity, such as those denoting family relations, father, mother, child, brother, sister; *secondly*, words denoting parts of the body, and material objects, for which children have names; *thirdly*, personal pronouns. which are found to be amongst the most durable parts of language; *fourthly*, the numerals, especially the first *ten*; *fifthly*, verbs expressive of universal bodily acts, such as, to eat, drink, sleep, walk, talk, &c.; *sixthly*, names of domestic animals. As no human family was ever yet without its own stock of such words, and as they are never changed, within the narrow domestic circle, for other and strange words, they are almost indestructible possessions; and it is only among tribes who have been entirely broken up and enslaved, so that family relations have been destroyed, that this domestic language can have been wholly lost. Tribes and families spread abroad have preserved them for thousands of years, in a degree which has allowed an easy recognition of this sign of a common origin.

A second class of words, which are common to nations who had attained some degree of refinement before the era of their separation, consists of terms connected with simple arts, such as simple nations early acquire, as, to plough, to weave, to sew; names of metals, of weapons, tools, articles of dress. It may be observed, that words of this class are often common to nations whose domestic vocabularies are different; and, on the other hand, often different when the domestic vocabulary is nearly the same.

A careful investigation of the phenomena of resemblance or analogy, which discover themselves on comparing different languages, on the principles to which I have now adverted, will go far towards an elucidation of the question, whether such phenomena of resemblance belong to the primitive and original parts of language, and therefore prove a common origin in the nations to which they belong, or are of later date, and are referable to intercourse, or conquest, or some secondary and contingent cause. Such an investigation will, at least, greatly aid and confirm the conclusions which we may draw from historical evidence of a different kind as to the history of tribes, and their mutual relations to each other.

I shall now advert to some of the principal instances in which ethnology has been extended through the medium of researches into the affinities of languages confirmed by historical facts.

Nearly the whole continent of Asia and Europe is divided between four great classes of languages; and in this instance history affords reason to conclude, with great probability, that the affinities of language really mark out as many races or great families of nations. These four sets of languages alluded to are, 1. The Indo-European Languages. 2. The Northern Asiatic, which, for reasons to be explained, I shall term the Ugrian or Tartarian Languages. 3. The Syro-Arabian or Semitic Languages. 4. The Chinese or Indo-Chinese, or the Monosyllabic or Uninflected Languages.

1. The name of Indo-European was first given some years since (by the writer of a review of Adelung's Sprachenkunde, in the Quarterly Review) to a group of languages which in-

cludes a great many of the principal idioms of Europe and Asia. It may be divided into several different groups. The first group, or the Classical (as it may be termed, for the sake of distinction), includes those languages in which are the chief remains of ancient literature; and these are more perfectly inflected, and have a more complete grammatical construction than the rest. They are three, viz., the Sanskrit, Greek, and Latin, which perhaps resemble each other nearly in an equal degree. The second group in this first class consists of languages very nearly allied to the Sanskrit, viz., the ancient languages of Persia and Media. They are, 1. The idiom in which the Persepolitan and other Persian cuneiform inscriptions are written, so nearly approaching the Sanskrit, that the meaning has been made out through this resemblance. 2. The Zend, in which the Zendavesta, or the Scriptures of the Fire-worshippers or followers of Zoroaster were written, is another language of this group, to which we may add the modern Persian. 3. The next branch, reckoning by the degrees of affinity, to the Sanskrit, is the Old Prussian family, including the Lettish and Lithuanian. The Lettish and Lithuanian are said to resemble the Sanskrit more nearly than any other European dialect; and Von Bohlen, who has written a work on this subject, assures us that he could compose whole sentences in Sanskrit, which would be intelligible to the peasants of Lithuania. 4. The Germanic family constitutes a fourth group. 5. The Slavic or Slavonic, or Sarmatian languages, are a fifth section. They comprehend the languages of the eastern parts of Europe, the Russian, Polish, Bohemian, and the dialects of a great part of the countries *in Europe* subject to the Turkish empire.

I shall now stop to inquire what inferences can be drawn from these philological facts.

We know from history, that the nations above mentioned have been spread, from a remote age, over the regions which they now inhabit. The Teutonic and Scandinavian tribes of the German race were known to Pytheas, on the shores of the Baltic, in the time of Aristotle, and the Brahmans who spoke Sanskrit, to Megasthenes at the court of Palibothra, supposed to be Patna, soon after the same period. All ancient Germany, Sarmatia, Italy, Greece, Persia, Media, India, were then inhabited by independent races of people, speaking different languages, but languages strikingly analogous and palpably allied to each other. The question which now occurs to be solved, is, whether these circumstances prove the nations themselves to have descended from a common origin, or admit of any other explanation? Foreign conquests have often introduced new languages among nations; but it is hard to conceive any such hypothesis applicable to the facts now under consideration. If we suppose an Asiatic tribe, speaking any one idiom belonging to this dynasty of languages, to have made conquests ever so extensive in Europe, without leaving any traces in history, which is next to impossible, we could not imagine that they would introduce the German language among the German race, and the Slavonic among the widely-spread natives of Sarmatia, the Greek among the Greeks, the old Italic among the ancient nations of Italy. Any person who considers the nature of that deeply-rooted affinity which exists between these languages, will find convincing proof that their analogies are not engrafted, but spring out of their very fundamental structure. If we take into account the immense extent of the countries over which these nations were spread from so early a period, we cannot refer their affinity of speech to any circumstances accidental and necessarily of restricted and merely local influence. It must have been the result of a gradual deviation of one common language into a multitude of diverging dialects; and the conclusion that is forced upon us, when we take all the conditions of the problem into consideration, is, that the nations themselves descended from one original people, and, consequently, that the varieties of complexion, and other physical characters discovered among them, are the effects of variation from an originally common type.

Besides the languages which I have enumerated as the principal members of the Indo-European family, other groups have been more lately admitted, and some of them appear to be more remotely allied to that stock. One of these is the Celtic language, which was at one time asserted to be entirely distinct, and of separate origin from the Indo-European stock. Their affinity to that stock is now generally admitted, though some persons think that their descent is not genuine, and that they spring from an intermixture of an Indo-European with a more ancient aboriginal or perhaps a Finnish speech. In the east of Europe, the Skippetarians, or Arnauts, or Albanians, the descendants of the ancient Illyrians, and Epirots and Macedonians, speak a distinct idiom, which, by Ritter von Xylander, has been proved to be a particular branch of the great Indo-European stem. To the same stem belong the dialects of the Ossetes in Mount Caucasus, supposed to be descended from the ancient Avars, and those of the Lesgians, in the same mountainous region, the Armenians, all the Tâjîks or real Persians, and, lastly, the Affghans or Patans, who speak the Pushtû language, and constitute an intermediate branch between the Persians and the Indians, more nearly allied, however, to the latter, but still distinct from both. Thus we find the Indo-European family to comprise nations which are spread—

> Omnibus in terris quæ sunt a Gadibus usque
> Auroram et Gangen.

2. The dialects which belong to the second great dynasty of languages in both parts of the great continent of the world —for thus we may term Asia and Europe—are not so obviously allied as are those of the former stock. Yet the proofs of their affinity are on the whole sufficiently marked. They are spread abroad more widely even than the former languages, and occupy tracts lying to the northward, eastward, and westward, of the Indo-European countries. It is the opinion of many who have investigated these subjects, that the nations who speak dialects of the Ugrian or Tartarian family, were spread over vast regions of the world before the approach of the Indo-European nations, who drove them out towards the north, and east, and west. When the European nations, at a later period, approached them, they retired into the distant parts of Scandinavia, and in the Russian empire, beyond the Valdai Mountains, or the great Uwalli, a chain which divides the waters falling northward into the Baltic and Frozen Oceans, from those which, by a longer course, find

their way to the Euxine and Caspian. The Valdai Mountains were for many ages the boundary which separated the Slavonic Russians from the people of this second race, who occupied the northern border of Europe and of Asia. The tribes who now belong to this class of nations in Europe, are the Finns, the Lappes, the Magyars in Hungary, and a variety of nations spread through all the northern regions of Russia, from the White Sea to far beyond the Uralian Mountains.

On the high table-land of Asia, other great divisions of people constitute the main part of the Tartarian stock. They are the Turkish, the Mongolian, and the Mantchu-Tartars. It was from this family that all those nations were descended who poured themselves down, during all the middle ages, upon Christendom and the East, who first overran the caliphat and the Asiatic parts of the Byzantine Empire, and afterwards under Tchingis Khan, conquered all the countries intervening between the Sea of Japan and the Danube. The discovery of a real and deeply rooted affinity between the languages of these nations, was a more difficult enterprise than the tracing of relations between the Indo-European languages. The nations of High Asia who belong to this stock, have passed under the general name of Tartars, given to the followers of Tchingis. The tribes of analogous speech in the northern parts of the Russian empire are termed, by the Russians, Tchudes and Ugres or Ogors. Hence the name of Ugro-Tartar, which comprehends the whole family. The writers who have explored the history of these idioms are Dobrowsky, Gyarmathi, a Hungarian, Rask, Vater, Abel Rémusat, and lastly Dr Schott of Berlin. The evidence of affinity between these nations themselves is principally that afforded by their languages. It may, however, be deemed historical, since history affords proofs that no other explanation can be found of the phenomena ascertained to exist, except that of primitive affinity. We must observe, that the connection of these languages is not merely or principally a resemblance of particular words, such as might have been borrowed by one people from another. It is a deeply-rooted affinity in the original elements of speech, or in the primitive vocables, and a striking analogy in grammatical construction. But beyond all this, there is a singular resemblance in the structure of words themselves. For example, in all these languages that euphonic principle prevails which was first pointed out by Viguier in the Turkish language, and was termed the "quadruple harmony of vowels." According to this principle, only vowels of certain sets can occur in the same word. There are four such sets in the Turkish language, and this law pervades all the dialects of the Turkish race spread from the confines of China to Constantinople. It also prevails in the idioms of the Mongoles and Kalmucks, and in those of the Tungusian and Mantchu Tartars, who are masters of China. It has likewise been noticed in the idioms of the Finnish and Lapponic nations; and Mr Norris, the learned secretary of the Asiatic Society, who is one of the greatest linguists in the world, assures me that it is equally prevalent in the language of the Japanese, which is likewise spoken in the Lieu-kieu islands.

How far towards the west the offsetts of this race extended themselves is as yet unknown. Professor Rask and others have thought the Euskaldunes or Euskarians, or the ancient inhabitants of Spain and the South of France, who are supposed to have occupied those countries before the Celts, might be referred to this stock of nations, but no sufficient proof has been afforded in support of this hypothesis, nor does it appear at all established that the aborigines of Britain were a Finnish race, though this has been conjectured, and is, I believe, the opinion of Dr Meyer, who has studied the Celtic languages and literature more successfully than any of his contemporaries. On this subject I shall say nothing at present, since I hope that we shall soon hear it most ably treated by that learned writer, with some of whose works on the Celtic history I have had occasion to become acquainted. It is no small confirmation of his views to observe that, in many parts of Western Europe, the sepulchral remains of the oldest and most barbarous class of inhabitants display a type resembling that of the round-headed Tartar race. If these facts should be fully determined, we may find hereafter that the old British legend of Gog and Magog is at least true in a mythical sense. But this subject, though it involves the earliest history of our ancestors, is still involved in doubts which nobody has yet made any serious attempt to dispel, though it is within the reach of historical research.

On the present occasion, I have no opportunity of going fully into the ethnological results which present themselves on considering the history of the Ugro-Tartarian nations. I may just observe, that this survey brings together, and represents as branches of one stock, great tribes of people who differ physically from each other, as the Mongolians with broad lozenge-faced heads, flat noses, and projecting cheekbones, the various Turkish races, some of whom, as the Kirghises and other eastern tribes, resemble the Mongoles, while the Turks of Stamboul and Roum have a very different physiognomy; the little black-haired reindeer-feeding Lappes, and the phlegmatic fair-haired Finns ; and lastly, the proud and lordly Magyars, who have almost a Grecian physiognomy.

3. The third family of nations reckoned among the principal races of the great Continent, are the Chinese and Indo-Chinese nations. They are brought into one department by the resemblance of their languages, all of which consist of monosyllabic words, incapable of grammatical inflection, and likewise by their great physical resemblance and geographical proximity.

4. The fourth great family of nations before alluded to are the Syro-Arabian, or, as German writers term them, the Semitic nations, to which stock the ancient Hebrews and Assyrians, and Syrians and Arabs belonged. The Arabian branch has spread its language over all the countries formerly occupied by these nations. Late researches into the languages of Northern Africa indicate, that the Syro-Arabian stock extended originally much farther into that part of the world than was formerly supposed.

I shall not now attempt to enumerate all the great families of languages or of nations in the world, and perhaps I have already trespassed too long on the time and patience of the Society. I shall sum up what remains to be said in a few general observations on the principal divisions of the human family.

The languages of Africa are not sufficiently known to be accurately classified or referred to particular groups, which we may consider with any degree of certainty as comprising the whole number. We may, however, reckon several great families. These are, 1. The North-African languages, more or less connected with the Syro-Arabian idioms. To this department are referrible with different degrees of evidence, 1. The Abyssinian languages, the Ghiz, Tigre, and Amhárá languages, and perhaps also the Galla dialects spoken by nomadic nations through an immense space to the southward of Abyssinia, including the idiom of the Somáli on the eastern coast. 2. The Berber, Kabylian, and Shillah languages, which are dialects of the ancient Lybian. Professor Newman, who has studied these languages with greater success than any other person, considers them as a branch of the Syro-Arabian family, correlative with the ancient Hebrew, Phœnician, and Syrian.

A third division in this North-African family is the idiom of the Haúsa Negroes in the inland parts of Africa or Súdan. This language, as Professor Newman has proved, has grammatical affinities to the Syro-Arabian languages.

A second African family of languages, of perhaps equal extent, are the Kafir languages. More or less of affinity, both in words and in grammatical structure, pervades all the known languages of the black woolly-haired nations to the southward of the equator, including all the Kafir nations, the Suaheli on the eastern coast, and the nations of the so-termed Empire of Congo. It must be observed that some of the tribes belonging to this division have Negro features, while others have the Kafir figure and a physiognomy of a very different type.

3. The language of the Hottentots and Bushmen constitute a third group.

4. The languages of the Negro nations of Western Africa. The most correct enumeration of these languages as yet made, classified according to these vocabularies, is that which

was laid before the Ethnological Section of the British Association by Dr Latham, two years ago, at York.

The languages of America, as yet generally known, belong to one type as far as grammatical structure is concerned, and that a very peculiar one, and strongly marked. It is possible that exceptions may hereafter occur to this remark, and Dr Buschmann, the learned editor of Baron William von Humboldt's posthumous works, who has for several years been employed in preparing for the press a work on the American languages, written partly by William Humboldt, and partly, as I believe, by himself, informs me that he has found exceptions to the general character of these languages, though he does not consider as such the Othomi language which Naxera, a Mexican writer, supposed to be a monosyllabic idiom, and therefore very unlike the polysyllabic and polysynthetic languages, as they have been termed, of America. Several of the learned cultivators of philology, who have done credit to the rapidly increasing literature of the United States, have succeeded in classifying the native languages of North America, and referring them to a comparatively small number of families of great extent. These families are regarded by M. Gallatin as distinct from each other in relation to their vocabulary. Dr Latham, who has attentively studied their vocabularies, is of opinion that there is a greater connection between the different mother-tongues of the American nations, if we may use the expression, than M. Gallatin supposed. On this question I am unable to offer an opinion; but certainly the probability is on Dr Latham's side, since it is very unlikely, though perhaps the fact is not without example, that languages which have so great a similarity in structure as the American idioms, should yet display no proofs of affinity in their vocabulary.

I need not observe that the conclusion to be drawn in regard to the community or diversity of origin between the different American languages, is one very interesting in an ethnological point of view, especially if we take into account the very considerable physical differences which separate some of the American nations, as the Esquimaux, from rest.

It is a matter of still greater interest to determine wheth the American idioms bear any analogy to those of the O Continent sufficient to furnish an argument of common orig It has been observed that the Euskarian idiom, the old Iberia probably the oldest known language of the west of Europ has a resemblance in structure to the dialects of the Americ nations; and an American writer, Dr M'Culloh, has argue from this fact, that the nations of the New World had an European origin. But there are great diversities as well a resemblances between the Euskarian and the America languages, and nothing certain can be concluded from this argument, which, however, must not be entirely overlooked. It is observable that the races of people in the extreme north-east of Asia, beyond the river Kolyma, are described as differing considerably in physical conformation from the nations of Great Tartary. Very little is known of their languages; and it might possibly lead to some important discoveries if we could compare these unknown idioms with those of the hunting tribes on the opposite parts of America, particularly the Athabascas, and the nations bordering upon them. It has been long ago ascertained, that one language is common to the two continents, I mean that of the Esquimaux and the Fishing Tchuktchi in Asia, whose idioms certainly belong to the same stock.

In the South Sea there are, as I have observed, several distinct races. The most important of these are the Polynesians, descended from the Malays, from whom they differ physically. The history of the Papua races is very interesting. They resemble, in some particulars, the Africans of the Gold Coast, but differ widely in the shape of their heads from all the Negro races. Some Papua nations, whose idioms have been examined, have been found to speak dialects of the Polynesian language. Among them are the Figians. We may conclude that they are descended from Polynesians, though probably not without intermixture with people of the black or Papua race.

Australians constitute a third race in these regions. lialects appear, from the researches of Captain Gray, a particular family of languages, displaying great es, but bearing traces of affinity among themselves, rived from a common origin. One of the most inter- observations as yet made respecting them is the re- f Mr Norris, who has discovered indications of con- between the Australian dialects and the Tamulian, by the aboriginal inhabitants of the Dekhan.

ve endeavoured to explain what I believe to be the les on which ethnological research must be conduct- ve would maintain for that study and its results the ter of a really scientific and philosophical pursuit. ly certainty that can be obtained in the formation of and families of nations, must be founded mainly on cal proofs. We must begin by establishing the histori- t of relationship or consanguinity between tribes of before we venture to refer them to one race, or to their diversity of origin. The deviations which are to have taken place within the limits of particular s, are too great to allow us to assume diversity of on the mere ground of physical difference; and it is obvious, that we cannot assume a near relationship simple evidence of physical resemblance.

ould carry me far beyond the limits of the subject of aper if I were to attempt to sum up any general re- or trace the bearing of facts on the great question of ity or diversity of human families; but I may be al- to conclude with this remark, that the farther we ex- he various paths of inquiry which lie open to our re- es, the greater reason do we find for believing that no ountable line of separation exists between the now ified races of men; and the greater the probability, judg- ne from such data as we possess, that all mankind are ded from one family.

On the Motion of Glaciers. By M. CH. MARTINS, in a Letter to Professor JAMESON, Editor of the Edinburgh New Philosophical Journal.

SIR,—In Mr J. D. Forbes' fourteenth letter on glaciers, which appeared in April 1847 in your valuable Journal, I observe, at page 330, the following expression :—" The opinion of MM. Dollfus, Martins, and Collomb is, that seasons and weather make no difference whatever on the motions of the glacier of the Aar." On no occasion have I expressed the opinion Mr Forbes here ascribes to me. I have even affirmed the very opposite, in the course of Geology which I had the honour to deliver this winter to the Faculty of Sciences at Paris. On considering what may have led Mr Forbes into this error, I can think of nothing else than an expression in a letter addressed to M. Arago, and inserted in the Comptes Rendus of the meeting of the Academy of Sciences of Paris on the 26th October 1846. At page 824, it is stated :—" *From the* 18*th of August at noon, till the* 29*th at half past six in the evening*, the glacier has advanced 1^m. 874, or 0^m. 173 in twenty-four hours. This progress has been uniform by day and night, independent of changes in the temperature, of rain or snow ; in a word, of all atmospheric modifications." It will be seen that there is no reference here to the uniform progress of the glacier throughout the course of the year, but only during eleven days of the latter portion of the month of August 1846.

Allow me, Mr Editor, to make another remark. At the commencement of his thirteenth letter on glaciers, Mr Forbes gives an account of an experiment made by him on the glacier of the Bois, and by M. Dollfus and myself on that of the Grünberg, for the purpose of measuring the relative speed of the surface and the bottom. He asserts the priority of his experiment, and also of its publication in your Journal on the 1st October 1846. I think it due to my fellow-labourer M. Dollfus, at present in Spain, to mention the fact, that an account of our experiment and its results had already been published by a friend in the Supplement to the Gazette d'Augsbourg on 17th September 1846; (*Beylage. zur Algemeinen deutsichen Zeitung*, No. 260, p. 2079) and consequently two weeks before Mr Forbes' account appeared.

CH. MARTINS, D. M. & G.

Paris, Quai de Bethune, 1847.

On the Colour of the Water of Glaciers. By M. ED. COLLOMB.

I have had occasion, many times, to observe the waters of the torrents which issue from the principal glaciers of Switzerland; those of the two Grindelwalds, the Rosenlaui, the lower and upper Aar, the glacier of the Rhone, that of Viesch, Aletsch, Hinter Rhein, the valley of Saas, and the valley of Chamouni. In no instance that has come under my observation have the waters proceeding immediately from the melting of the glaciers presented a blue tint; I say *immediately*, because we can now speak only of the water as examined at a short distance from glaciers. After it has traversed a considerable space, spring-water becomes mingled with it, and when it is collected in a tranquil basin, forming a lake, it is no longer the water of the glacier, but has undergone important modifications. Its constitution and colour are no longer the same.

On issuing from the glacier, the water is always turbid and milky; it is never limpid, as MM. Agassiz and Ch. Martins have already remarked; and, with respect to colour, it is always of a greyish hue. This tint, well known to observant artists, varies infinitely; its scale is very extensive, from greenish-grey to bluish-grey or yellowish-grey, with a great variety of shades. The colour of this water is subject to atmospheric influences; during rain, it is not of the same hue as in clear weather. I could judge of the variations in colour it undergoes, while making a series of regular observations at the terminal vault of the glacier of the Aar, in 1845 and 1846, for experiments in gauging.

In fine weather, when viewed from a distance in a mass, it has a grey, tarnished, and opaque appearance; it holds a great quantity of mineral particles in suspension. During rain, the grey changes to ochre-yellow; when examined in a flask or glass, the difference of hue partly disappears. All this is explained if we take the origin of this water into consideration. It is derived from four different sources: 1st, from the ice; 2dly, from the superior nevé; 3d, from rain; 4thly, from atmospheric condensation, (the spring-water which may rise under a glacier is insignificant). Before reaching its terminal point, it performs an important part in the economy of the glacier. By means of canals and capillary fissures, it circulates throughout the entire mass, and carries along with it the bodies it encounters in its passage.

Independent of the mineral particles whose origin is known, it likewise holds in suspension a great quantity of organic fragments, both vegetable and animal. The Mers de Glace of the high alpine regions are inhabited by a multitude of organised beings, as yet little known. Thus MM. Ch. Vogt and Bassnitz, who have entered upon investigations (partly unpublished) relating to the red snow, which is very abundant on glaciers, have discovered under the microscope, and I can myself verify the fact, that besides the sporules of *Protococcus* which constitute the red snow, the waters of glaciers contained a very great variety of fragments of hitherto undetermined cryptogamous vegetables. I have no doubt that further researches will enable us at a subsequent period to introduce a glaciary flora into science.

With regard to animal organisms, M. Desor was the first to make known the existence of glacier fleas (*Desoria glacialis.*) They are found over the whole extent of the surface. On the glacier of the Aar, we have only to lift the first stone from the median moraine to discover myriads. These small insects are so numerous in their icy dwelling, that it acquires a blackish hue from them. They are unable to subsist on distilled water; they must therefore be supported by some organic remains. We have tried to feed them with the stalk and sporules of the protococcus without any conclusive result. Inclosed in a vessel surrounded with a cold mixture, they are unable to support a temperature of —18 centigrade degrees. This degree of cold kills them in a few seconds.

These facts ought to be impressed on the memory of those who have occasion to visit glaciers; they shew that there is a peculiar organisation to be studied in such places. A numerous series of microscopic beings belonging to the vegetable and animal kingdom live and prosper in the bosom of the ice, at a height of 2500 metres above the level of the sea.

On the other hand, it is not less clearly proved, according to the laws which regulate the movements of glaciers, that the entire mass renews itself at the end of a certain number of years, and contains the mineral, vegetable and animal remains which exist on their surface and in their interior; they all reach in succession the terminal talus, and are again found in the waters which flow from the inferior vaults. Hence the origin of their great impurity, and the grey, milky colour, without transparency, which characterises them.—*Comptes Rendus de l'Academie des Sciences*, t. xxiv. p. 1093.

On the Parallel Roads of Lochaber, with Remarks on the Change of Relative Levels of Sea and Land in Scotland. By DAVID MILNE, Esq., F.R.S.E., Member of the Wernerian Natural History Society; of the Geological Society of London, &c.

There are few questions in geology which have given rise to so many theories, and so much speculation, as the origin of the parallel roads in the valleys of Lochaber.

In the year 1817, the late Dr MacCulloch gave an elaborate description of them, in a paper read before the Geological Society of London. In the year 1818, Sir Thomas Dick Lauder read before the Royal Society of Edinburgh a paper, full of equally interesting details. Both *of these observers* suggested, in explanation of the shelves which mark the mountain sides of these valleys, that th had been occupied by lakes, which, by earthquakes or other viol convulsions, had been drained. This theory was generally received, until, in the year 1839, Mr Darwin, so justly celebrated as a geologist, and an accurate observer, published his views, and pronounc the shelves to have been formed by the sea; an opinion which, b sides being rested on proofs derived from the locality, he enforc also by his observation of similar appearances in South America.

Mr Darwin's opinion has received the assent of Sir Roderick Murchison, Mr Lyell, and Mr Horner, all successively Presidents the Geological Society, besides other geologists, both at home an abroad, who are justly regarded as authorities in physical scienc Relying on the soundness of their views, I confess that when I we to Glen Roy, in the year 1845, it was with a strong conviction, th the lake theory was indefensible; a view to which I was the more i clined, from having studied certain marks along different parts the Scottish coast, on both sides of the island, which satisfied m that the sea had recently stood at a much higher relative level th at present; and that, in its recession, it had formed, all round o coasts, shelves or beach lines, very analogous to those in the Loc aber valleys. I had not been two days in Glen Roy, before I sati fied myself that these views were inapplicable to the shelves in and its associated valleys. But I was unable during my visit 1845, to remain long enough to obtain evidence of the manner in which the lakes had been dammed up, and eventually drained. I therefore resolved to defer the farther consideration of the subject, until I could pay a second visit. This I accomplished in September 1846, when I spent a week in the examination.

In the following paper, I shall attempt to explain my reasons for thinking Mr Darwin's theory inadmissible, and to point out the manner in which, as it appears to me, the lakes were drained,—not as supposed by Dr MacCulloch and Sir Thomas Dick Lauder, by convulsions of nature, but by the gradual operation of ordinary causes.

Though it is the principal object of this paper to account for the formation of the Lochaber shelves, there are no views regarding them which can be suggested, which have not a more general bearing, and the soundness of which may be tested by evidence supplied from other sources. Former writers, accordingly, and especially Mr Darwin, have felt it to be necessary, after giving their explanation of the parallel roads, to shew, that the principles on which it rests, are, at least, not inconsistent with any established truths in other branches of geology.

I shall not shrink from subjecting the Lake theory, which I have to submit, to a similar ordeal; and the more so, as I feel satisfied that it receives great support from geological considerations now held to be well established.

As the whole details of the parallel roads have been fully described by former writers, I shall limit myself to points on which I have obtained new information, or with regard to which doubts have been expressed.

1. One of the points of the class last referred to, is the absolute horizontality of the shelves. Mr Darwin, referring to Sir Thomas Dick Lauder's observations on this point (p. 76.), hints at the possibility of errors and omissions in the calculation. M. Bravais, in his paper on the lines of former sea-level in Finmark, suggests, " that an accurate geodetic levelling should be applied in the case of the *doubtful lines* in Scotland," evidently refering to Glen Roy. Mr Horner, the president of the Geological Society, in his last year's address, observes ; " Mr Darwin's explanation of the parallel roads of Glen Roy, that they are ancient sea-beaches, appears to be now generally accepted; and it would be most interesting, if it were ascertained by exact levellings, such as those of M. Bravais, *whether they really are parallel.*" Similar doubts had been expressed by Sir R. I. Murchison, Mr Horner's predecessor, in his anniversary address of 1843; in support of which, he refers to the concurrent opinion of M. de Beaumont and Professor Phillips.

In accordance with the doubts expressed by these authorities, the Geological Section of the British Association, at their last meeting, agreed on an address to Her Majesty's Government, requesting them to cause the parallel roads of Lochaber to be examined by the officers of the Ordnance Survey, to ascertain their supposed horizontality.

I have no doubt that the result of this official survey, if made, will be to establish the absolute horizontality of the shelves. In August 1844, Mr D. Stevenson, at my request, was so obliging as to examine them, and the conclusion at which he arrived, is explained in a letter to me, from which I make the following extracts:—" I have had a number of levels taken, the particulars of which I shall give you afterwards. The *result*, I think, *leaves no doubt as to the perfect horizontality of the 'roads.'* The glen is much more extensive, both as regards length and breadth, than I anticipated, and the height of the roads above its bottom is also very considerable, and any thing like a series of cross sections, referred to the same datum, would be a work of very great magnitude; a month, I should say, would not complete it. The whole we have been able to do, therefore, is to test the uniformity of the levels of the different roads, by viewing them with a good instrument from several points, as was done by Sir Thomas Dick Lauder; and, in addition to this, a section was made along the middle road, where it is pretty well defined from Glen Turret downwards, for a distance of nearly 3½ miles, and throughout that stretch, the road was found to be *perfectly horizontal.*" ... " If I had seen that any thing further could be done, I would have left my assistants for a few days longer; they were there a week."

These observations of Mr Stevenson, whose professional accuracy is undeniable, confirming, as they so completely do, the result of Sir Thomas Dick Lauder's survey (and he, too, was aided by an engineer,) leave no doubt in my mind as to the horizontality of the roads. It is scarcely necessary to refer to any farther and weaker testimony on the subject. But it may be proper to add, that during the two occasions when I visited Glen Roy, I had a pocket-level with me, which I constantly used; and that on the last visit I was accompanied by Mr R. Chambers of Edinburgh, who had a larger spirit-level, and we never could detect any deviation from horizontality.

2. There is a point of some importance bearing on the theory of the shelves, about which former observers have disputed. MacCulloch found by his barometric observations, that the Glen Gluoy uppermost shelf is 12 feet above the highest in Glen Roy; but he attributed this difference to errors of observation, and his theory in regard to the formation of the shelves proceeds expressly on the assumption that these shelves are precisely on the same level. Sir Thomas Dick Lauder mentions, however, that Mr M'Lean, the engineer who assisted him, made the Glen Gluoy shelf 12 feet above that in Glen Roy, whilst Sir Thomas himself made it 15 feet. According to the observations made by myself and Mr Chambers last September, the difference is much greater. By levelling, we made it 29 feet; by joint barometric and sympiesometer observations, I made it 23 feet.

3. Whilst on the subject of Glen Gluoy, I may mention that I discovered in it a second shelf, which the barometer shewed to be 200 feet, and the sympiesometer 213 feet, below the level of the one before referred to. I detected it first immediately above the mouth of Glen Fintec. It is traceable on both sides of the glen, and for several miles upwards.

4. There is a circumstance of great importance, in the theory of these roads, on which I was so fortunate as to obtain farther information. I allude to the fact, that most of the shelves are coincident with some summit level, so as to admit of the waters flowing over that level as over a lip. Thus the uppermost shelf of Glen Gluoy No. 1, in Sir Thomas Dick Lauder's Memoir, is (as he explains) exactly coincident with the water-shed ridge which divides Glen Gluoy from Glen Roy, so that the waters (whatever they were) which stood at that height and formed the beach No. 1, must have flowed out at the head of Glen Gluoy into Glen Roy. In like manner, the uppermost shelf in Glen Roy, No. 2 in Sir Thomas Dick Lauder's Memoir, is (as he also mentions) exactly coincident with the water-shed ridge which divides Glen Roy from the valley of the Spey; so that the waters which stood in Glen Roy at No. 2 beach, must have flowed over the head of the Glen into Spey valley.

In like manner, the only shelf which occurs in Glen Spean, No. 4 in Sir Thomas Dick Lauder's Memoir, is exactly coincident with, or rather is a few feet above, the pass of Mukkul at the head of Loch Laggan, through which pass, the waters standing at the level of No. 4 must have flowed eastward into Spey valley. These coincidences, as Mr Darwin admits, " are so remarkable, that they must (I use his own words) be intimately connected with the origin of the shelves; although such relation is not absolutely necessary, *inasmuch as the middle shelf of Glen Roy is not on a level with any water-shed.*" (P. 43.)

The middle shelf here alluded to is No. 3 in Sir Thomas Dick Lauder's list. The discovery which I made, was its exact coincidence with a water-shed at the head of Glen Glaster, a glen which, though branching up from Glen Roy near the bottom of it, oddly enough does not appear to have been visited, and certainly not to have been described, by any former observer.

Shelves 3 and 4 are the only shelves which enter and run up this glen. Sir Thomas Dick Lauder's map inaccurately represents shelf 2 as marking it on both its sides. Shelf 2 stops, however, on both sides of Glen Roy a little to the eastward of, or above the mouth of Glen Glaster.

In following shelf 3 to the head of this glen, I found that it was there lost in a low mossy flat. A little beyond this flat, and a few feet below the summit-level, an *old river-course* can be distinctly traced down a slope towards Loch Laggan. It has a rocky bed, over which a great body of water had evidently flowed at some former period. The breadth of the rocky bed is from 30 to 40 feet; the knolls of rock are from 2 to 5 feet high, and amongst them are rounded blocks of stone, such as occur in all great Highland rivers. I traced this rocky channel for about a mile towards Loch Laggan; and I afterwards found the place where it had discharged its waters into Loch Laggan, when that loch stood at shelf 4. It is marked by a huge delta, forming a projecting buttress at the level of that shelf, and bulging far beyond the general side of the Laggan valley.

On examining the rocky knolls attentively in this ancient river-course, I found that the smooth faces were all towards Glen Glaster, and the rough faces in the opposite direction, affording proof, if such were needed, that the stream which flowed there had come from Glen Glaster.

A small rivulet trickles now among the rocks, infinitely too feeble to have produced the appearances.

It is now, therefore, established, not only that the whole of the 4 shelves of Lochaber are coincident with water-sheds respectively, but that a great body of water had filled Glen Glaster, and of course Glenroy, the outlet of which was down this ancient revercourse to shelf 4 in Loch Laggan, which is at a lower level by 212 feet.

Whilst on this subject, I may mention farther, that I examined narrowly the interval of space between shelf 1 at the head of Glen Gluoy, and shelf 2 at the head of Glen Turret, where the last shelf is nearest to Glen Gluoy. This space also appeared to me to exhibit the features of an ancient river-course, though they are not so striking as those just described. The distance from the one shelf to the other, is about a mile. Where the Glen Gluoy shelf ends, rockly knolls rise above the moss, water-worn below the level of the shelf, but rough above that level. Their smooth faces are all towards Glen Gluoy. Near shelf 2, in Glen Turret, the rocks have evidently been excavated and cut into by some considerable stream; at present a very small burn runs in this rocky channel, quite incapable of producing the appearances.

The grandest exhibition of an ancient and deserted river-course is, however, at the head of Loch Laggan. The Pass of Mukkul is a channel, the bed and sides of which are entirely rock. It is, at its narrowest part, about 70 feet wide, the wall faces being on each side from 40 to 50 feet high. The rocks at the sides are evidently water-worn for about 30 feet up. To the eastward, this gorge expands into a broad channel of several hundred yards in width, divided in the middle by what has formerly been a rocky islet, against which the waters of this large river had chafed in isuing from the pass. For nearly a mile towards the east, the rocky banks continue on each side, but they gradually diverge, having between them a mossy flat sloping gently eastward. The smooth faces of the rocks within the probable reach of the river-waters, are all towards the west, where Loch Laggan is situated. The height of shelf 4 above the highest point of this deserted channel, is, by barometric measurement, about 21 feet, which affords, therefore, some probable estimate of the average depth of the river. I have only to add, that no stream whatever now occupies this water-course, except where, for a short part of it, the river Pattaig flows in a reverse direction into the head of Loch Laggan. This stream was, when I visited it last September, only about 18 inches deep and 30 feet wide, and must be quite inadequate to have formed the rocky banks on each side of it.

The ancient river-course now described is of much greater size than that at the head of Glen Glaster, just as the Glen Glaster river-course is of greater dimensions than those respectively at the head of Glen Gluoy and Glen Roy. The reason is obvious. The river at Mukkul had to discharge not merely the waters which belonged to Glen Spean, but also those which flowed out from Glen Glaster, comprehending Glen Roy, Glen Collarig, and Glen Gluoy. The Glen Glaster river-course discharged the waters of Glen Collarig, Glen Gluoy, and Glen Roy, whilst the Glen Gluoy stream discharged only the waters of one lake. Mr Darwin did not visit the Pass of Mukkul. If he had studied the appearances presented by it, and by those almost as strikingly exhibited at Glen Glaster, he would have found it impossible to deny that the waters which formed shelves 3 and 4 flowed down river-courses, and therefore could not be arms of the sea.

His proposition is, "that the waters of the sea, in the form of narrow arms or lochs, such as those now deeply penetrating the western coast, once entered and gradually retired from these several valleys;" and he adds, that after considering the "several and successive steps of the argument, the theory of the marine origin of the parallel roads of Lochaber, appears to me *demonstrated*." (P. 56.) I regret that Mr Darwin should have expressed himself in these very decided and confident terms, especially as his survey was incomplete; for I venture to think, that it can be satisfactorily established that the parallel roads of Lochaber were formed by fresh water lakes.

1. The first circumstance which I shall notice as fatal to Mr Darwin's theory, is suggested by the fact last referred to, that the waters which formed the different shelves must have *flowed out of the glens, and descended by river-courses to lower levels.* The waters which formed No. 1 shelf in Glen Gluoy descended nearly 29 feet by flowing into Glen Roy. The waters which formed No. 2 shelf in Glen Roy flowed in like manner into the valley of the Spey. The waters which formed No. 3 shelf were discharged over the head of Glen Glaster, down a slope of about 212 feet in vertical height, into Glen Spean. Lastly, the waters which formed shelf 4 in Glen Spean, issued out of Loch Laggan by the ancient river-course at Mukkul.

Now, any *one* of these cases is irreconcilable with the notion that the shelves had been formed by arms of the sea. There is no such thing in nature as a river flowing out of an arm of the sea, to a lower level.

Mr Darwin, as we have seen, admits that this coincidence of the shelves with water-sheds, must be in some way *connected* with their origin; and, accordingly, he endeavours to give an explanation of it consistently with this theory. He says that these water-sheds are *land straits*, with sea on each side of them, and that they consist of littoral deposits or accumulations of matter formed by the opposition of tides. This opinion, however, is altogether inconsistent with the actual circumstances of the case. In the first place, there is at these water-sheds no accumulation of littoral deposits or detrital matter. They consist, generally, of bared rocks, forming sloping channels or water-courses. In the second place, there is no trace of water at the same level, on each side of these water-sheds. In the third place, when land straits are formed by the accumulation of matter from opposition of tides, it is not in situations like the heads of glens, which narrow to a point, and at that point are separated by a small neck of land,—it is where there is space for a considerable current on each side of the strait.

For these reasons I consider that Mr Darwin's explanation of the coincidence of the shelves with the water-sheds before described, is quite inadmissible.

2. The second serious objection to Mr Darwin's theory arises from the fact, that *the shelves* in the different glens *are not coincident in level.* If they had been formed by arms of the sea, as the land rose out of it, the sea should have formed lines in all the valleys which it entered, at precisely the same levels. But neither of the Glen Gluoy shelves is to be seen in any of the other valleys. So also the No. 2, or highest shelf of Glen Roy, and the next lowest, or No. 3, do not occur in the lower part of that glen, or in the adjoining valleys of Glen Glaster, Glen Spean, and Glen Treig.

Mr Darwin attempts to explain one, but one only, of these circumstances, viz., the difference of level between No. 1 and No. 2 shelves, by a theory of very questionable soundness. He says, that the tide in Glen Gluoy may have risen 20 feet higher at the head of the estuary, than at the head of Glen Turret. It would be necessary that it should rise 29 feet higher. But if this were the case, then the shelves, at all events, in Glen Gluoy, would not be horizontal or nearly so;—they would have sloped upwards towards the head of Glen Gluoy, by 29 feet in the course of 6 or 7 miles,—the length of the glen. But this would be inconsistent with the great and well-established fact so characteristic of these Lochaber shelves; and moreover, though the beach-lines at the heads of the two glens might not be exactly coincident in level there, they ought, at all events, to be so at the mouths of the glens where the supposed arms of the sea joined the main body of the ocean,—which is not pretended.

This theory, however, would explain merely the non-appearance of shelf 1 in Glen Roy. The non-appearance of all the others is accounted for by Mr Darwin, simply by supposing that something or other had prevented them being *marked* in the other glens.

In support of this view, Mr Darwin refers to two intermediate shelves which are faintly traceable on Tombhran and elsewhere, in order to shew that the water did produce marks at some places, and not at others. But, from the faintness of those intermediate lines, it is manifest that the water had stood at their level for a much shorter period than at the levels of the principal shelves; and, therefore, no fair inference can be drawn from the former applicable to the latter.

3. These considerations suggest, however, a separate and even a more serious objection. Not only should the sea have made markings at the same levels in all the Glens of Lochaber, but it should

have produced *similar appearances, and at the same levels respectively, on all the mountains in Scotland*, high enough for the purpose. Mr Darwin says, "that it would be more proper to consider the *preservation* of these ancient beaches *as the anomaly*, and their obliteration from meteoric agency the ordinary course of nature." (P. 60.) Supposing him right in this, he ought to have shewn how circumstances caused that anomaly at Glen Roy and its adjoining valleys. But he has not shewn, and cannot shew, that the sides of the Glen Roy mountains, are in any respect different from those other highland mountains. Indeed, he has himself pointed out a similar beach line at Kilfinnin, in a glen towards Inverness. I take leave farther to doubt the soundness of Mr Darwin's proposition, that the preservation of ancient beach lines is anomalous. The whole of Scotland, and I believe also of the British Islands, is begirt with lines of ancient sea beach.

4. The ancient sea beaches, now alluded to as existing along our coasts, present a very marked contrast with the Lochaber shelves. If these shelves had been formed by the sea, it will, I presume, be admitted that, considering their great altitude, they are of much older date than beach lines at a lower level. *If older, then they should be less perfect and entire.* But the contrary is the case. They are incomparably more perfect and entire than any of the lowest ancient sea terraces which occur along our coasts.

5. If the Lochaber roads were formed by the sea, the well-known action of the tides, to which Mr Darwin refers, would have *precluded* the formation of them *along lines absolutely horizontal.*

Mr Darwin refers to a case in South America, where, in 18 miles, the tidal wave rises at one place 20 feet higher than another in the same estuary. Nearer home, in the Bristol Channel, the sea rises at its head about 50 feet higher than at its mouth.

The tide at Blackwall rises 12 feet higher than at Yarmouth. In the Frith of Tay, the tide rises at Perth 18 inches above the level at Newburgh. The tide at Alloa is said to rise 2 feet 9 inches above its level at Leith. At Glasgow, the tide rises 10 or 11 inches above its level at Greenock. On the Dee, the level of high water is, at Chester, 8 inches above what it is at Flint, near the mouth of the river, a distance of 11 miles.

On this principle, the beaches of Lochaber, if formed by arms of the sea, ought all gradually to rise to the head of the Glens—narrowing, as these glens do, towards the head. But this is negatived by the fact.

6. On more narrowly considering the effect of tidal action, it will readily occur, that the beaches formed by the *sea* must be materally different from those of a *lake*, in which there is no movement of the water at the sides, except such as is caused by winds common to both. In the case of the sea, there is not only a vertical rise and fall of water (which, on the west coast of Scotland, is from 8 to 16 feet)

twice in the 24 hours, but also a good deal of lateral current alternately in opposite directions. Hence the *sea*, whilst it will eat into the land more rapidly than a *lake*, will also spread out more completely the detritus washed down into it. In a lake, on the other hand, which has no movements of water either vertical or lateral, the detritus deposited on the sides of a valley occupied by it, will be scarcely if at all removed, and will thus form projecting buttresses nearly flat in their upper surfaces, and presenting steep escarpments towards the lake.

Now, applying these two principles of tidal action to the shelves of Lochaber, we seek in vain for any actual *indentation* into the sides of the hills. The shelves consist entirely of *buttresses* which stand out from the sides of the mountains ; and these buttresses, so far from sloping at an angle little less steep than that of the sides of the mountains (which should be the case with the sea), form flats or terraces which deviate in general very slightly from the horizontal.

7. If the shelves were formed by the action of the sea, *they should be most distinct at places where the hill sides had been most exposed.*

Thus, on the north and north-west sides of Craig Dhu, and on the west side of Bohuntine, where there must, on Mr Darwin's theory, have been an open expanse of ocean, the shelves should have been most distinct. But at *these places, the three highest shelves are entirely absent ;* the fourth alone is visible, though, being the lowest, it must have been less exposed. It is quite anomalous, on the marine theory, that the shelves should not have been formed where the force of waves and of tidal currents must have been greatest, and that they should have been most distinctly formed in the higher and more sheltered parts of Glen Roy.

The hills at the mouth of Glen Roy seem rather to indicate that the highest shelves had not been formed on them,—the very reverse of what might have been anticipated if Mr Darwin's views are sound. If they had been formed, they would not have been obliterated, as is manifest from the perfect preservation of shelf 4 on Craig Dhu and Bohuntine.

8. Having stated these objections to the theory of Mr Darwin, I proceed to consider his objections to the theory that the shelves were formed by lakes.

These objections resolve entirely into the difficulty of explaining the disappearance of the barriers, which must have dammed back the water in the valleys. But it would be no good reason for rejecting an explanation founded on the existence of barriers, even though we could not very clearly account for the disappearance of them, provided that there is direct and conclusive evidence that such barriers existed. Now, I conceive that there is such evidence furnished by the considerations before referred to.

Let us examine, however, the alleged difficulty of explaining, how the waters could have been dammed up in the valleys to the height of the several shelves.

Shelf 2 is distinctly marked on both sides of Glen Roy, down to a certain point,—and also on both sides of Glen Collarig, down to a certain point. At this period, the water flowed from the east end of Glen Roy into the valley of the Spey. Something must have existed, therefore, in both glens at the points above referred to, to prevent the extension of the shelf westward.

Shelf 3, in both glens, extends a little more to the west than shelf 2. We have seen that, whilst Glen Glaster is exempt from shelf 2, it is well marked on both sides by shelf 3.

To explain these facts, I assume that there was a blockage of some sort, in Glen Roy, which filled the lower part of the valley up to the level of shelf 2, and which blockage extended a little farther east than the mouth of Glen Glaster. I assume also a similar blockage in Glen Collarig, which filled the lower part of the valley, and as far eastward as the place where shelf 2 stops in that glen. This blockage may have been gravel, clay, or any other detrital matter.

Such is the supposed state of things, whilst the waters stood at shelf 2 in Glen Roy; at which period, it will be remembered, they were discharged to the eastward.

Former writers have assumed, that when the waters sunk from shelf 2, the amount of sinking must have been 82 feet, the distance of shelf 3 below shelf 2; and that this sinking had been one act, caused by an earthquake, or other violent operation, which all at once lowered the barrier by that number of feet. But this is a mistake. MacCulloch takes notice of a shelf faintly marked on Tombhran hill, between shelf 2 and shelf 3, though he expresses afterwards some uncertainty about it. In fact, there are two intermediate shelves visible there; and they are also discernible, at precisely the same level on Ben Erin, and also more distinctly near Achavaddy, on the south side of Glen Roy; the one being about 14 feet below shelf 2, and the other about 36 feet lower down.* These two intermediate shelves clearly indicate, that the water which filled the valley, did not all at once sink from shelf 2 to shelf 3. They prove that the water first sunk down 14 feet, and was stationary at this level for some time; that it then sunk down other 36 feet, and continued at this level for some time; and that it again sunk other 32 feet, at which level it remained for a much longer period, till it formed shelf 3.

It is evident, from these facts, that the lowering of the barrier (of whatever material composed) which confined the water in Glen Roy, was a process of a more gradual and ordinary description than what former writers, and especially Mr Darwin, suppose. It is plain, also that the barrier which kept in the waters was less rapidly worn down, when they stood at shelves 2 and 3, than at either of the intermediate levels. We see that at shelves 2 and 3 the waters flowed over rocky ledges; in the one case into Spey valley, in the other case by Glen Glaster. Is it not fair from this to infer, that at the intermediate shelves, the water flowed over a blockage of such a nature as was capable of being more easily worn down and obliterated, such as detrital matter? It is, at all events, obvious, that when the water sunk 14 feet, the discharge must have ceased at the east end; and that it henceforward would go on at the west end, probably near the mouth of Glen Glaster. At every other place, the rocky mountain sides rise so high, as to preclude the possibility of overflow or attrition.

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* There are hummocks or knolls of stratified gravel and sand in Glen Glaster, the tops of which are all about 36 feet above shelf 3. It is probable that they were deposited when the lake stood at one or other of the intermediate points last mentioned.

Keeping these principles in view, let us suppose that the detrital matter which blocked up the lower parts of Glen Roy extended a very little to the east of the mouth of Glen Glaster. How easy it is to suppose that this detritus was scooped away, so as to allow of the recession of the waters westward, and of their flowing round the east jaw of Glen Glaster, and on towards the head of that glen, from which they would descend to Glen Spean? For this purpose, it is not necessary to suppose that there was any *lowering* of the supposed barrier in level, even by a single foot. All that is required is the scooping or wearing away of the detritus, so as to allow of the extension of the lake a little to the westward;—a few yards would be sufficient. As the discharge at this first sinking, must have been at the west end, it is fair to infer that the wearing away of detritus took place there; and when once a flow of water was established through detrital matter, the process of removal would go on rapidly, so as to allow of repeated sinkings of the lake, till it reached the water-shed at the head of Glen Glaster, the rocky nature of which would for a time stop any farther sinking, and thus allow of the formation of shelf 3.

According to the foregoing views, we see how the waters would, by successive steps, sink from shelf 2 to shelf 3, and, after entering Glen Glaster, form a marking on both of its sides. We see, also, that the same removal of detritus which allowed the formation of shelf 3 in that glen, would allow also the extension of it on Bohuntine Hill, beyond the point where shelf 2 terminates.

Whilst this process of attrition was going on in Glen Roy, there need have been no contemporaneous change in the blockage of Glen Collarig. But there also, at some time or other, a similar scooping out of detritus must have taken place, to allow of the extension of shelf 3 beyond the point where shelf 2 terminates.

Nor is it difficult to conceive how this removal of detritus was effected. Thus, in Glen Collarig, there are, on both sides of the glen, burns of considerable size and power (from the steepness of their channels) which flowed into the lake. There are three of them which now descend in that part of the glen marked by shelves 2 and 3. If the detritus which formed the blockage in the lower part of the valley consisted of the same loose sand and gravel which now abounds there, forming cliffs from 70 to 80 feet high, nothing is more easy or natural than the scooping of it out by such means.

The same observations apply to the blockage in Glen Roy, which, to prevent the waters when at shelf 2 flowing into Glen Glaster, must have been near the mouth of Glen Collarig, called Gap in the maps, out of which, from the number of streams in it, a considerable current had flowed.

So far with regard to the first depression to shelf 3, at which period I suppose the Collarig blockage to be still existing (scooped out a little towards the west), and the blockage in Glen Roy to have been, by a similar process, removed below the mouth of Glen Glaster. The next well marked shelf is No. 4, which is seen on Craig Dhu and Bohuntine, and on both sides of Glen Collarig, and which infers the necessity of removing the blockage entirely from both Glen Roy and Collarig.

This may have been, as in the case of the previous depression, a gradual operation. There is no improbability whatever in the ultimate removal by rivers and burns, of a blockage of the nature supposed. There flows into Glen Roy, from Bohuntine Hill, and at or near the very place where the blockage must have existed, the Tundrun Burn, the sides of which shew mica-slate rocks cut through by it to the depth of about 70 feet, and detrital matter above these rocks cut through to the depth of 130 feet. If, since the drainage of the lake, it has thus cut through and removed blockage to the depth of 200 feet, of which one-third is solid rock, this rivulet must have had nearly equal power to wash away the more superficial blockage which existed at this place previously to that event.

The same observations apply to the detrital matter in Glen Collarig, which could easily be carried away by the numerous mountain torrents flowing into that glen.

The following is the manner in which Mr Darwin alleges that the two depressions must have taken place, according to the lake theory. He says, that there are two barriers, one in Glen Collarig, and the other in Glen Roy : " Let one of the two barriers, we will say the smaller one in Glen Collarig, *give way from the effects of an earthquake*, or other cause, the lake will now stand at the level of the middle shelf, the barriers having *given way* 82 *feet vertically*. Again let it burst, and *this* time rather *more than* 212 *feet vertical must be swept away*. Let all this have taken place, but still a barrier nearly a mile long and 800 feet in height is left standing across the mouth of the Roy. Must we suppose that *each time* the barrier in *Glen Collarig* failed, the one in *Glen Roy gave way the same number of feet*, through some strange coincidence ?" It is plain, from this representation, that Mr Darwin had not in his vie the more simple and gradual process of removal which I have ve tured to suggest. It is not in the least necessary to imagine, th there was any sudden sweeping away of barriers of the magnitu supposed; and which would certainly imply the existence and opera tion of some stupendous agent; but the effect of which would, as M Darwin truly says, have also probably obliterated the shelves. Th process which I have suggested, implies the continuous working o ordinary and natural agents,—agents which are now seen at this very place, producing results similar to those required.

Mr Darwin says, that the barrier across the Roy must have been 800 feet high. This is on the assumption, that the valley of the Roy was then of its present depth and form. But is there to be no allowance made for the removal by the river Roy of detritus from the valley? It is manifest, from many appearances along its sides, that the river Roy has cut down at least 200 feet below what was the original bottom (whether of lake or estuary,) formed when the waters stood at shelf 4; so that the height of the supposed barrier to retain the waters at shelf 2 would not exceed 600 feet above the bottom of the valley, and might be much less, if the valley were more filled up. Mr Darwin considers it probable (p. 53,) that the buttresses existing on the sides of Glen Roy indicate, that the valley, upwards from Bridge of Roy, had been filled with detrital matter to the very level of shelf 4; in which case the blockage or barrier requisite to form a lake at the level of shelf 2, would have been only about 300 feet above the bottom of the valley. My belief, however, is, that the whole not only of the lower part of Glen Roy, but also of the district about Unachan, High Bridge, and Fort-William, was blocked up with detrital matter, which, in the course of time was washed away by rivers; and that, when the blockage of Glen Roy was removed, the depressed waters standing at shelf 4 were dammed back by detrital accumulations near Unachan, so as to force a a discharge by the Pass of Makkul. This 4th, or lowest shelf, seems to me to stretch much farther to the north, on both sides of the Spean, than former observers have noticed. On the hills flanking the east side, this shelf can be traced to within nearly a mile of Spean Bridge. On the opposite side of the valley, it can be traced to within 6 or 7 miles of Fort-William. The width of the valley where this shelf on both sides ceases to be visible is about 4 miles. Across the mouth of this valley, a little beyond a line joining the extreme visible points of shelf 4, lies the high and elongated hill of Tomnempearaichin, the top of which I found, by the level, to be only 50 or 60 feet below shelf 4; and there is no great difficulty in imagining that the whole of this district, as far as Fort-William, where the enclosing hills are greatly higher, was filled by detritus. There are, even now, detrital remnants of enormous size, of which the well-known Hill of Tomnahurich at Inverness (about 180 feet high and half a mile long), and a hill to the west of it (240 feet high), are specimens indicating the prodigious accumulations once existing in the great glen.

To this point I shall revert. But, in the mean time, taking for granted that such detritus did fill the lower parts of the valleys, it is easy to understand how it should have dammed up the waters into lakes, and how, by a gradual and long-continued process of wearing down, this detrital blockage should have been lowered to the requisite extent.

I have endeavoured to explain the damming back and the depressing of the lakes to their successive levels, without imagining that the level of the sea was then different from what it is at present. If the sea stood at a higher level, then the difficulties of the explanation become less; because the valleys must then have been previously less excavated than they now are, by the operation of rivers. There are good reasons for believing, that since the period of the deposit of the boulder-clay in Scotland, the sea has stood at least 1000 feet higher on the land than at present. Of course, it must have been after the land rose out of the sea to some extent, that the Lochaber shelves could have been formed by lakes; but the lowest of these might have existed when the sea stood 900 feet above its present level, in which case the depth of detrital matter required to dam up the valleys would be comparatively small.

I have attempted to explain how the valleys of Glen Roy, Glen Collarig, and Glen Spean were blocked up. There still remains Glen Gluoy, which, as before mentioned, contains two shelves, one of which is about 29 feet above the highest of Glen Roy. Glen Gluoy being unconnected with the other valleys, requires a separate blockage. There would be no great difficulty in imagining the existence of detrital blockage in this glen, at the place where its shelves terminate towards the west, as it is generally, throughout its whole course, exceedingly narrow; and being unconnected with Glen Roy (though MacCulloch states the reverse), its blockage may have been worn down at periods, and in a way independently of Glen Roy and Glen Collarig.

Before, however, forming a very decided opinion as to the position of the blockage applicable to Glen Gluoy, I should like to examine more particularly than I was able to do, some of the other Glens which open into the Caledonian valley on both sides, with the view of ascertaining whether they contain traces of horizontal shelves about the same height. Mr Darwin takes notice of one in the valley of Kilfinnin,* about 10 miles to the eastward, and which he says is (by his barometric observations) about 40 feet above the highest shelf in Glen Roy; in which case it would be only 10 or 11 feet above that in Glen Gluoy, a difference quite within the limits of error.

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* It is to be regretted that Mr Darwin, when he visited Lochaber, was not provided with a spirit-level. His statement as to the horizontality of this shelf at Kilfinnin, depends entirely on ocular inspection and barometric measurements.

I have observed several places along the Caledonian Canal, where there are traces of one or more horizontal terraces, at a height of from 650 to 690 feet above the sea. From these considerations, I infer the possibility of there having been a blockage which applied not merely to Glen Gluoy, but to other glens opening into the great Caledonian valley; and it would, therefore, be most important, that future observers should turn their attention to the adjoining districts.

My explanation of the Lochaber shelves depends entirely on the accuracy of the supposition that the valleys were, in the lower parts of them, filled up with detrital matter, capable of being gradually worn down and washed away. This supposition is not only not improbable on general principles, but is verified to a great extent by the remains of such detrital matter at and above the heights required for it. Thus, in Glen Collarig, there are to be seen, near the east end, and within about half a mile of the place where the blockage must have existed, enormous heaps of boulder-clay, gravel, and sand. These detrital deposits must have existed in Glen Collarig before the shelves were formed, because shelves 2 and 3 are seen distinctly indented upon these deposits; and I was particularly struck with the fact, that these deposits reach to a height of more than 100 feet above shelf 2. Here is proof, that in Glen Collarig, before the formation of the lake which filled it, there was detrital matter of sufficient depth and consistency to have retained water at the required height. At the place where shelf 2 terminates in this glen, the valley, even at present, is only about 236 feet deep, and 300 yards wide, so that the depth of detrital matter does not exceed the limits of probability—nay, is exemplified by the occurrence of much larger accumulations of detritus in all parts of the Highlands.

It is here proper to explain, that there are in these valleys, as elsewhere in Scotland, two distinct sorts of superficial deposits,—the one consisting of the well-known boulder-clay, and the other of ordinary gravel and sand. This bloulder-clay exhibits the same general characters, which it commonly possesses elsewhere; it is unstratified, exceedingly obdurate, of a dark-bluish colour, and filled with water-worn boulders. This boulder-clay I found at the following places;—Spean Bridge, where it is covered by sand; Bohuntine Hill, where it is covered with laminated clay, sloping to the centre of the valley, and about 250 feet below shelf 4; Bohina, on the south side of Glen Roy; Inverlair Bridge, near Loch Laggan; Glen Glaster (on the west side of the valley), from 50 to 80 feet *above* shelf 3; Glen Collarig (near the Gap), where it rises *above* shelf 2; Glen Gluoy, as seen at the water-shed between it and *Glen Roy*, and *on a level with shelf* 1. The deposit occurs also at

Clenichan, at the river Roy, where the mica-slate rocks, through which the river now runs, are covered immediately by boulder-clay, —the boulder-clay being here covered by deposits of irregularly stratified beds of gravel and sand, from 150 to 200 feet thick. At this place, I observed among the boulders in the hill, granites (with red and grey varieties), old conglomerate, and red porphyry,—rocks, all of which must have come from a distance.

From the fact that this boulder-clay occupies alike the highest and lowest parts of the glens; and, more especially, that in several places it is seen distinctly covered over by laminated clay as well as by stratified gravel and sand, it may be inferred that the boulder-clay, with its imbedded blocks, was deposited, certainly not after the drainage of the lakes, but either before the valleys were occupied with water, or during that period.

In regard to gravel and sand, I do not remember having, in Glen Roy or its contiguous valleys, observed any considerable beds of it, so high up as the boulder-clay. But at lower levels, there are everywhere enormous cliffs of it to be seen, several of which I measured, and found to exceed 180 feet in height. These cliffs are formed out of the ancient bottom of the lake or estuary which filled the valleys, and are composed of materials washed down from higher levels. The adjoining mountains of the district afford ample evidence, that gravel as well as boulder-clay had been, by some cause or other, brought and deposited over all this country, filling the valleys to heights exceeding the highest of the Glen Roy shelves. Thus, on the turnpike road between Tyndrum and Inverournan, near the summit level between the two valleys, which I estimated to be about 1030 feet above the sea, there is great abundance of sand and gravel. On the Black Mount, about 4 miles north of Inverournan, and at a height of 1300 feet above the sea, there is an immense accumulation of gravel and boulders, particularly on the south side of the summit. In the high ground north of Dalwhinnie, which I estimated at 1200 feet above the sea, there are great heaps of gravel, forming mounds and ridges. These facts, taken into connection with the undoubted fact, that detrital matter has been spread over the greater part of Scotland, to a height of at least 1500 feet above the sea, pretty clearly indicate, that detrital matter not only may have been, but actually was spread over the Lochaber district, and filled its several valleys, to the height of at least the highest of the Glen Roy shelves, thus affording ample blockage for its lakes.

I may mention that there are, in this part of the Highlands, several lakes of small size, at very high levels, the existence of which renders the lake theory of the Glen Roy shelves less improbable than to some it may appear. Thus, at the well-known pass of Rest-and-be-Thankful, there is a small lake, which is about 800 feet above the sea, and there are traces of its having stood formerly from 40 to 50 feet higher. To the south and west of Loch Treig about 3 miles, there are two considerable lakes, one called the Lake of Corry, and the other called Benofflap, which appear, from the accounts received of them, to be about 1200 to 1300 feet above the sea. There are several also on the Black Mount, at about the same high level.

Before concluding what I have to say regarding the parallel roads of Lochaber, I may briefly notice the theory, that the lakes which filled them may have been confined by glaciers, or by the moraines of glaciers.

This was one of the districts which, in the opinion of Agassiz and Buckland, afforded undeniable proofs of the existence of glaciers. The former published a paper* on the subject, in which he says: "When I visited the parallel roads of Glen Roy with Dr Buckland, we were convinced that the glacial theory alone satisfies all the exigencies of the phenomenon; and as this locality is the best known, I may limit myself to this example for the explanation of all others."

M. Agassiz, in the paper now alluded to, explains the grounds on which his theory rests; and it is accompanied by a plan of the locality.

It appears to me, (1.) That the facts on which M. Agassiz rests his theory are incorrect. (2.) That, assuming as true the facts stated by him, they still afford no evidence that glaciers existed in the Lochaber valleys.

(1.) There are three main facts relied on by M. Agassiz. He states, *First*, That in Glen Roy, and in that part of Glen Spean between Bridge of Roy and Loch Treig, there are 3 shelves visible; *Secondly*, That these shelves all terminate on both sides of the valley at or near the Bridge of Roy; *Third*, That the bottom of Glen Spean, in front of Loch Treig, is not only polished with that polish characteristic of glaciers, but is, moreover, scratched transversely,—that is to say, at right angles to the direction of the valley, by a cause which evidently proceeded from Loch Treig.

To explain these appearances, it is suggested, that "the supposition of a great glacier descending from Ben Nevis, and shutting up the valley of the Spean, *by resting on Moeldhu*, which is opposite, combined with the influence of a glacier from Loch Treig, and which would bar the valley a second time at that height, *would explain all the facts.*"

These facts, for an explanation of which this theory was invented, appear to me not to have been accurately observed. In the first place, the *three* shelves do not occupy, as M. Agassiz asserts, "all the sinuosities of the lower part of Glen Spean, and of the whole of Glen Roy." It is only the lowest of the three shelves which occurs in Glen Spean, and in the lower part of Glen Roy. The two uppermost shelves stop short of the mouth of Glen Roy by about 2 miles, so that, if the Lake in Glen Roy was dammed back by a terminal moraine, that moraine could not have rested on Moeldhu, at the foot of Glen Roy, but must have been pushed up that valley, before the Ben Nevis glacier, 2 miles farther,—an operation which the levels, distance, and direction of that valley would have rendered impossible.

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* Jameson's Edinburgh New Phil. Journal, vol. xxxiii. p. 236.

In the second place, the shelves do not, as M. Agassiz says, "terminate at the same point,"—viz., at Moeldhu, where he supposes the terminal moraine of the Nevis glacier to have been. The two uppermost shelves (as just stated) do not come within two miles of this point; and the lowermost shelf, instead of *terminating* there, runs, as formerly explained, several miles northwards, on both sides of the valley towards Unachan, where they are 4 miles apart. It is scarcely necessary to say, that a moraine in this low district, which is not connected with any Ben Nevis valley, and considering its required height and length, is inconceivable.

In the third place, as to the existence of transverse scratches on the rocks in Glen Spean, which are said to indicate the movement of some body from Loch Treig, I could see no such scratches, though I twice surveyed the ground, and narrowly inspected the rocks, especially at the outlet from Loch Treig. Indeed, the supposition that any glacier flowed out of Loch Treig, seems to be almost excluded by the fact, that a shelf, perfectly horizontal, exists on both sides of the narrow outlet from Loch Treig, and continuously into Glen Spean Such a shelf could not have been formed, and would have been obliterated by any glacier moving out of Loch Treig.

(2.) But assuming all these facts to be as M. Agassiz states them, do they present unequivocal proofs of the movement of glaciers, and the formation of moraines? Scratches on polished rocks may be made by various causes; and if a moraine existed on Moeldhu, surely some trace of it, or of the great blocks which generally accompany moraines, would have been particularly observable there;—whereas there is scarcely a block or a patch of gravel to be seen in that part of the valley.

Farther, I would observe, that the valley supposed to have been the birth-place of the glacier which produced this Moeldhu moraine, is about two miles distant from Moeldhu, with an undulating country between them, which is most unlikely to have formed the channel or bed of a glacier, Dr Buckland and M. Agassiz speak of this glen as connected with Ben Nevis. But here, again, there is apparently some mistake. The valley in question is Larich Leachich, and runs up, not in a NW. direction towards Ben Nevis, but in a SW. direction towards the head of Loch Treig. It is an extremely short glen, and rises to no great height.

Finally, supposing that if, in spite of all these objections, it were allowed that a glacier had moved down this little valley, and across the very uneven country to Moeldhu, so as to block up Glen Roy and Glen Spean, it would still remain to explain the blockage of Glen

Gluoy, which, by no possibility, could be accounted for by a moraine at or near Moeldhu.

That there are certain appearances in the valleys of Lochaber, which must have been produced by attrition of some kind, I am free to admit. Water, accompanied by gravel and other detritus, appears, however, to have been the agent, and not ice. At the Monessie Falls, the valley is compressed to a narrow gorge, and the rocks forming the east side, present evident marks of attrition on a large scale, the rough faces of the rock being all down the valley. The rocks are here covered by sand and gravel, which indicate the flowing of water and of drift at that height, when these rocks were worn down. In like manner, at the outlet of Loch Treig there are immense expanses of rock, all smoothed and rounded on the sides facing the SW. or WSW. by compass,* These smoothed rock-faces prevail to a height of about 786 feet above the lake, and 1680 feet above the sea, above which level they are no longer visible. There are many boulders lying on these smoothed surfaces, all of rounded forms. That these boulders have come from the west, is evident from the nature of them ; several of a pink coloured felspar, having been traced by me to a dyke of the same peculiar rock a few hundred yards to the west, from which they had evidently been derived. Another circumstance proved this still more strikingly. In one place, a few hundred feet above Loch Treig, I observed a series of rocky knolls, in an east and west line. The parts of these knolls which were smoothed and worn down were uniformly to the west, whilst their rough facts were all to the east. It was clear, on an inspection of these knolls, that they had been worn down on their west sides ; and the smoothed sides were so close to the knolls respectively to the west of them, that nothing except some fluid, charged, it may have been, with drift, could have possibly reached and acted on them.

This last point was still more palpable in several places where there were narrow smooth-sided troughs, more or less steep, on the sides of hills. These troughs had apparently been natural fissures in the rocks, which had been smoothed by the long-continued action of water, for the notion that ice could have entered and rubbed them, was entirely precluded by their narrowness, situation, direction, and other circumstances.

M. Agassiz, in the paper before alluded to, says that he will never forget the impression he experienced "at the sight of the terraced mounds of blocks which occur at the mouth of the valley of Loch Treig, where it joins Glen Spean. It seemed to me (he adds) as if I were looking at the numerous moraines of the neighbourhood of Tines, in the valley of Chamounix." These terraces of blocks, thus likened to moraines, are, I presume, the accumulations of blocks on the lowermost horizontal shelf, which is very conspicuous at the entrance to Loch Treig on both sides of the valley. On this shelf there are multitudes of blocks, just as in many other parts of the valleys, where this shelf and the others occur. But this fact is perfectly consistent with the theory that these shelves were formed by water, and, indeed, can be explained on no other, when it is considered that they form at Loch Treig, as at every other place, a line absolutely horizontal,—a quality which, I presume, no moraine ever possesses.

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* The general line or axis of the lake is north and south by compass, the upper part being towards the south, so that the motion of a glacier down this valley would have smoothed all the *south* faces of the rocks. It is also important to remark, that, on the west side of the lake, the rocks facing the lake are, as compared with those on the other side, exceedingly rough, shewing still more clearly that the smothing agent had crossed the valley of Loch Treig, in a direction *not* parallel with its longer axis, but obliquely to it.

The only place where I observed an accumulation of blocks, at all resembling a moraine, is on the east side of Glen Spean, near a place called the Rough Burn, about three or four miles to the north of Loch Laggan. The accumulation is enormous. Blocks are piled over each other to such a height as to render the general surface of the moor, over a wide extent, quite undistinguishable. This accumulation occurs not at the mouth of any valley. On the contrary, the hills near these blocks on the east side, are not much furrowed even by mountain torrents, and present a somewhat steep and high wall face to the west. On looking round for any possible explanation of the occurrence in this spot of so unusual a quantity of boulders, consisting almost entirely of gray granites, whilst the rocks on which they lie are different, I could not help noticing that the valley on the opposite or west side presented an opening or depression, though at the distance of 2 miles. This opening is the outlet of Loch Treig, and bearing about WSW. by compass. The appearance of the locality at once suggested the probability that the blocks had in some way issued through this opening, and had been transported across the valley to their present situation, where their farther progress was arrested by the lofty hills forming here the east side of Glen Spean.

I have already stated reasons for thinking that no glacier issued from Loch Treig. The only alternative seems to be the agency of water.

I proceed now to show that the lake theory of the Lochaber shelves, and the principles on which I have endeavoured to account for the formation of lakes, and the eventual depression and drainage of them, are not inconsistent with any established geological truths, —but, on the contrary, receive support from collateral considerations.

1. The first circumstance which I shall notice, *is the occurrence of Parallel Roads in other valleys similar to those of Lochaber*, the formation of which can be attributed to no other cause than lakes.

I have the less hesitation in availing myself of this argument, when I find Mr Darwin adverting to traces of shelves at Kilfinnin, and in the valley of the Spey, in support of his theory.

But if Mr Darwin's views are sound, traces of shelves should not be confined to the two localities just mentioned; they should be visible in other parts of the country of equal height as the Lochaber mountains.

On the other hand, if it should appear that there are in many valleys, distinct beach lines, all horizontal, and presenting no uniformity of height above the sea, the argument against a sea theory will be strengthened, whilst a strong analogy will arise to favour the lake theory,—if these beach lines, precisely similar in all essential features to those of Lochaber, can, from their inland situation, and other circumstances, be clearly shewn to have been produced by the waters of lakes.

I proceed therefore to mention a few localities out of many, where phenomena similar to those of Glen Roy are observable.

(1.) At Inverournan (about 40 miles SW. of Lochaber) there is a lake called Loch Tulla, about 3 miles in length, and 1 in breadth. A stream enters from its east and west ends. Its surplus waters are discharged from its south side, by the river Urchay.

Two years ago, I discovered all round this lake indications of three levels at which its waters had stood, the lowest being about 183½ feet, the second 277 feet, and the highest 474 feet, above their present level.* Loch Tulla I roughly estimated at 540 feet above the sea. This lake, therefore, extending originally to about 6 miles in length and half a mile in breadth, had sunk 197 feet,—at which level it had stood long enough to form the second shelf; it next sunk 93½ feet,—when the third shelf was formed; after which it sunk 183½ feet—viz., to the present level of the lake.

It is unnecessary for me to enter into the proofs, that what I am now describing are really beach lines. Their perfect horizontality, which I ascertained by a spirit-level, looking at them from 12 or 15 different places along the banks of the lake,—their general conformity in sweeping round headlands, and retiring into valleys or burn-courses,—and the extent of flat surface at the levels of the different shelves, afford convincing and irrefragable proofs.

The difficulty here, as in other similar cases, is to discover, what could have dammed up the lake so much above its present level. The blockage, whatever it was, must have existed somewhere in the valley through which the river Urchay flows. The country, on all other sides of Loch Tulla, rises much higher than 500 feet above its present level. The two lowest shelves are traceable for some distance down the valley of the Urchay,—the middle shelf for about half a mile, and the lowest considerably farther. My notion is, that this valley had been formerly filled with a great accumulation of gravel diluvial debris, which was gradually eat away and lowered by stream which issued from the loch. Accordingly, there exist at and near Urchay Bridge, great heaps of unstratified gravel, h clearly present only a remnant of what must have formerly ed. The valley at this place, is a quarter of a mile wide ; and ides rise far above the required level.

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* These measurements were made by a mountain barometer, checked by the *sympiesometer*.

.) In the valley, at the head of which Tyndrum is situated, are very manifest indications of the beaches of an ancient lake, ugh the valley is now occupied by only an insignificant stream. Strathfillan church, the lowest terrace is about 50 or 60 feet e the stream, and may be traced continuously for at least a mile the valley. The stream has cut through this old lake bottom, iting beds of gravel, sand, and clay, which have been deposited arranged by the water. About 237 feet above this flat, there on the sides of the hills on both sides of the valley, traces of a ontal shelf, which can be distinctly followed with the spirit- from above Tyndrum village, down the valley by Auchreach houses, Enich farm-houses, and as far as Crianlarich toll. At al places, boulders appear to have accumulated on this higher Tyndrum is about 740 feet above the sea.

) Along the margin of Loch Awe, and particularly near Dal- r, there is a flat or terrace about 40 feet above the present level e lake ; and which manifestly indicates a subsidence of its water at depth.

) Along the margin of Loch Lubnaig, in like manner, there is or terrace about 40 feet above the lake, and which is very e on both sides. Here as well as in the former case, the flat back from near the margin of the lake to the mountains forming ide of the valley ; and the steep sides of which, contrast most icantly with the almost horizontal flatness of the ancient and ex- bottom.

Loch Lubnaig, the flat can be traced for a considerable way th sides of the valley, beyond the point where the lake now rges itself, and, indeed, almost as far as Leny. At this place ll as at Callendar, there exist indications of enormous quantities avel, which, before being cut down and carried away by rivers, ed ample means of blocking up the waters of Loch Lubnaig to ier level. The quantity of gravel which formerly existed here- s, may be inferred from the existence of the following rem-

out ¼ mile west of Callendar, there is a ridge of gravel and about 100 yards long, and from 40 to 50 feet high. Near it, is a conical mound of the same materials, and about the same t, bearing a thriving plantation. The ridge of gravel to the f Callendar, designated in guide-books as the Roman Camp, is y a remnant of the ancient gravel-bed with which the whole r was filled ; and when it contained a lake, of which there are abundant indications, it is probable, that, when Loch Lubnaig stood 40 feet above its present level, its waters were discharged into a lower lake, of which the eastern margin may be seen near the Lodge of Gart-House. Ultimately the gravel heaps which held in this Callendar lake on the east, had been cut through, so as to allow of its drainage; and, accordingly, there are, on each side of the river Teith at this place, gravel banks and cliffs from 70 to 80 feet high.

After the Callendar lake was drained, the waters which flowed out of Lock Lubnaig would acquire fresh cutting power, and would rapidly eat away the barrier which dammed back the lake to the higher level before referred to. Callendar is about 270 feet above the sea.

(5.) In the valley in which the town of Huntly stands, there are two terraces, the one about 32 feet above the other, which are very clearly the beaches of a lake, which has sunk from the one to the other, and latterly been drained off.

(6.) A few miles south of Inverury, there are distinct traces of a lake which formerly filled the valley. The burgh of Kintore has been built in the ancient bottom of the lake. There are two well-marked beach-lines round the whole valley; the one about 78 feet, and the other 50 feet, above the channel of the united streams of Don and Urie, which flow through the centre of the valley. The ancient bottom of the lake has been cut up by rivulets at the sides of the valley into separate fragments, some of them of so unusual a form as to have suggested a notion that they are artificial; and, accordingly, in the guide-books, and even in the recent statistical accounts of the parish, they are so described. Two of these alleged remains of antiquity are known by the names of Bass and Konin Hillock; and are variously conjectured to have been formed for sepulchral or judicial purposes. A similar mistake has been made with the hills of Dunipace, near Falkirk, which are represented by historians as formed to celebrate and record a peace between the Romans and the natives of Scotland. They are detrital remnants fashioned into conical shapes by the action of streams.

(7.) In the valley of the Leader (Berwickshire), there will be found traces on the hill sides, which clearly shew the action of water. Three very distinct markings of this nature are traceable near Dodd's Mill, at Hounslow, at Carfrae Mill, and at Annfield near Channelkirk. The terraces at these different places, judging by the sympiesometer, seem to be all very nearly on a level; and if, on a more minute survey, they really prove to be so, it would follow, that the whole of Lauderdale had formerly been one vast lake, with a blockage at or near Chappel. The height of these shelves is about 800 feet above the sea.

It is scarcely necessary to advert to the inland situation, and other circumstances characteristic of the various beach-lines now mentioned, to shew that they could not have been formed by the sea, but must have been produced by lakes which filled the valleys, and which sunk at different periods,—in most cases, disappearing altogether.

If, then, the existence of lake-beaches be so common in the valleys of Scotland, there will be the less hesitation in ascribing the Lochaber shelves to the same cause,—established as that cause has been separately by local evidence.

That the occurrence of lake-beaches in the valleys of Scotland should be frequent, is only what every geologist must be prepared to expect, who considers the proofs which may be adduced of the gradual emergence of the land out of the sea. Some of these proofs, in so far as afforded by Scotland, I shall immediately notice; but assuming that Scotland was, to the depth of 1300 feet or more, submerged beneath the waters of the ocean,—as it rose out, there would be lakes in every inland hollow, each, of course, having its river to carry off to the sea, the rain falling on its surface and that of the adjoining mountains. The stream thus issuing, would gradually wear down the detritus which formed a barrier at one end of the lake; and the cutting power of the stream would be gradually increased, as the elevation of the land proceeded; so that in most cases the blockage of lakes would, in the course of time, be extensively undermined and worn down, and sudden depressions of lakes would take place, leaving marks of horizontal shelves along the sides of valleys.

The progress of these important changes is indicated, in many parts of the country, by the existence of haughs or river-flats, far above the present channels of the streams, and which evidently had been formed when they flowed at a much higher level.

Thus, from Perth up to Loch Tay, a number of isolated flats or terraces occur, forming a pretty uniform level, rising gently inland, and at a rate rather faster than the slope of the river. Near Perth, these old haughs are from 90 to 100 feet, and at Dunkeld about 110 feet, above the river. This old haugh at Dunkeld may be traced on both sides of the valley,—Dr Fisher's house being on it at the east side, and Claypotts farm-house on it at the west side. It may even be traced a considerable distance up both sides of the Braan, where it slopes a little to the eastward.

There is a low haugh at Dunkeld, which is only about 20 feet above the present bed of the river, and is, therefore, quite distinct from the higher terrace above described. The ground is now cultivated and enclosed; so I suppose that the floods never rise to a level with it now.

On the Tweed, in like manner, the remains of ancient haughs can be traced in many parts of its course. About half a mile above Berwick Bridge, one may be seen on the south side, from 30 to 32 feet above the sea. At Gainslaw, it is 44 feet; opposite to Finchie, it is 55 or 56 feet; opposite to Paxton, it is 58 feet; at Norham, it is 93 feet above the sea.

At New Rattray (in the parish of Blairgowrie) I observed an extensive flat, or ancient haugh, with its cliff or bank about 80 feet above the River Ericht.

On the Isla, above Airley Castle, there is haugh land on both sides, about 30 feet above the present level of the river.

On the River Garry, about 3½ miles north of Blair, there are on the east side two terraces, the one about 30 and the other about 50 feet above the river; but whether they are the remains of ancient haughs, or the beaches of a lake, it is difficult to determine.

Remarks on the Trade Winds and other Currents in the Atmosphere, at Barbadoes; with an Attempt to Develop the Causes of Hurricanes in the West Indies. By ROBERT LAWSON, Assistant-Surgeon, 47th Regiment. Communicated by the Royal Scottish Society of Arts.

(Continued from page 154 of Volume xxxix. of this Journal.)

PART II.—*The Causes, &c., of Hurricanes.*

40. There are, at present, two views with regard to the phenomena of hurricanes, which have attracted a good deal of attention. One of these, advocated by Mr Espy, of Philadelphia, assumes that the aqueous vapour in a given column of the atmosphere becomes condensed, and that the latent heat of the vapour, which is consequently liberated, elevates the temperature of the surrounding particles of air, thereby rendering the whole column specifically lighter than the neighbouring portions of the atmosphere; and these, accordingly, in obedience to the laws of gravity, flow inwards from all points towards its base, while it rapidly ascends. On this supposition, the winds around the base of the ascending column should all flow in the direction of the radii of a circle, in the centre of which the base of the ascending column is placed. Professor Hare, of the University of Pennsylvania, has abopted the opinion of Epsey as to the ascending central column, and the centripetal motion of the surrounding air, but conceives that the upward current may, with more reason, be ascribed to the attraction of an electrified stratum of air,

operating on the denser stratum of the atmosphere in the vicinity of the earth, counteracting gravitation, and causing its transfer upwards.*

41. The other view has been maintained by Professor Dove, of Berlin, Mr Redfield, and Colonel Reid. According to it, hurricanes and great storms are gyratory, the wind flowing in a circle round a centre, and subsequently pursuing a direction, at every point of its course, perpendicular to what it would have followed were the opinions of Epsey or Hare correct. The facts adduced by Dove, Redfield, and Reid, all support the gyratory course of the wind in great storms, and are quite at variance with the views of Epsey and Hare.

42. Much misconception has been produced on this point, by supposing that the causes of the so-called whirlwinds, and of the hurricane, are the same, though they do not necessarily have any thing in common. The whirlwind is a phenomenon of very limited extent, which chiefly occurs during warm weather, when the air in contact with the earth is still, while the hurricane affects the atmosphere to a considerable extent, and only arises when there are opposite currents which are, obviously, intimately connected with its origin and course.

43. Mr Redfield, so far as I am aware, has not attempted to explain the origin of hurricanes. Colonel Reid thinks that they may be, in some manner, connected with the magnetic intensity, as they are frequent in the Caribbean and China Seas, where the magnetic intensity is very great, while at St Helena, where it is remarkably low, they are quite unknown.† This, however, is a mere coincidence, and does not seem in any way connected with the immunity of St Helena from storms, the real cause of which is its position in the midst of a large expanse of ocean, nearly equidistant from the polar and equatorial margins of the SE. trade-wind, and when the different superimposed currents in the atmosphere flow quietly onwards, without being liable to those sudden influences, or abrupt alterations of direction, which are so common in the Caribbean Sea, at least, about the inner margin of the trade-wind.

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* London, Edinburgh, and Dublin, Philosophical Magazine, vol. xix. p. 430.

† Reid on the Law of Storms, 2d edition, with additions, p. 509-10.

44. Professor Dove has attributed the gyratory character of hurricanes in the West Indies, to the resistance opposed by the NE. trade to the current from the SW. (about the inner margin of the trade), gradually deflecting the latter from its south-westerly direction to S. and SE.,* which change of motion being continued, would ultimately complete the the circle, the general direction of the wind coinciding with what is actually found to exist. Had Professor Dove been aware of the occurrence of a NW. current in the atmosphere, within the range of the NE. trade-wind, and of the gradual deflection this undergoes at its inner margin, under the influence of the attraction of the sun and moon, he would undoubtedly have materially modified his opinion.

45. Dr Young long ago ascribed the violence of hurricanes to portions of the upper atmospheric currents prematurely directed downwards, before their relative velocity had been sufficiently reduced by friction on, and gradual mixing with, the lower strata, dashing upon the earth with that tremendous force which gives them their distinctive character. Sir John Herschel favours this view.† The nearest approach, however, I have met with to the true explanation of the nature and causes of hurricanes, is given by Mr Howard: he says, "The present storm seems to have consisted chiefly in a prodigious aggravation (for the time) of the velocity and force of the currents, which ordinarily proceed to and return from the great equatorial stream. After an obstruction for some considerable time, of this interchange of air, causing oppressive heat at the Bahamas, rarefaction suddenly ensues to the SW. of these islands; a portion of the tropical air is rapidly elevated, and the air which supplies its place from the NE., from some cause not as yet apparent, flows in a *confined and accelerated* stream. That this stream, however, originates in the same cause with the ordinary trade-winds, and is destined to supply the void caused by rarefaction to leeward, is evident; for it commences with the rising sun, assumes its greatest force at the time the heat of the day is established, and ceases in the evening. This part of the hurricane flows with steady

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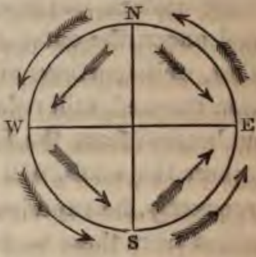
* Scientific Memoirs, Vol. iii. p. 209-10.

† Treatise on Astronomy, p. 132. Note.

unremitting violence; in which respect it agrees with our own north-easterly gales. When it is spent, *a stratum of air*, which had been interposed between it and the returning tropical current, suddenly shews itself; which having been balanced between these opposite impulses, obeys neither, but appears (as it is) at rest as to horizontal movement. But the calm is of short duration: the superior SW. wind comes down, and the moment it touches the earth, the storm is re-established."*

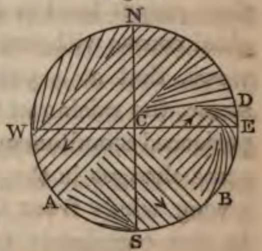
46. According to Reid and Redfield, the direction of gyration, in the West Indian hurricane, is contrary to the motion of the hands of a watch, or, is NW.SE., as indicated by the outer arrows of the accompanying figure. Thus the wind at N. would be from East, at W. from North, at S. from West, and at E. from South, and at every intermediate point, its line of direction would be a tangent to the circle at that point. The arrows in each quarter of the circle shew the general direction of the four different currents which are found in the atmosphere, at the inner margin of the trade-winds, which perfectly coincide with the facts which have been ascertained as to the gyration of the hurricane, and with which their connexion is too obvious to admit of a moment's doubt.

Fig. 1.



47. The directions of the wind, however, at different points of the hurricane, will be somewhat different from those in Fig. 1, though the general similarity will remain. For instance, in Fig. 2, supposing the centre of the storm to be at C, the trade-wind will be felt over nearly the whole northern half of the circle E N W., and will continue the same direction over a portion of the quadrant W S. And, in the quadrant E N, that portion of it which lies to the eastward of the line passing through the centre, will be

Fig. 2.



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* Howard's Climate of London, 2d edition, Vol. ii. p. 212-13.

gradually deflected from its original direction more and more to the eastward, as it approaches the point E. The NW. current, which naturally lies above the trade-wind, will first impinge on the earth's surface from A to C, along the S E. or central margin of the north-easterly current, in the quadrant WS., and will extend over the southern half of the quadrant S E. The SW. current, in like manner, flowing above that from the NW., will equally reach the earth along its north-eastern or central margin from B to C, and will be felt over the remaining half of the quadrant S E, and also over a variable portion of E N. Owing to the great diminution of pressure, in the centre of the storm, the lowest portions of the atmosphere, over the whole of the south-eastern quadrant, will be forced inwards towards it, and under the outer edge of the south-westerly current, so that, while portions of the trade, having their southerly motion checked, still retain their easterly direction, will be felt as breezes from the E., ESE., or SE.; other portions of air, nearer the south, will flow more nearly north, and will, consequently, be experienced as southerly winds; and the transition from these to those from the SE., or the deflected portion of the trades, at any given point, will occur gradually as the storm passes to the westward. Though not mentioned above, the great diminution of pressure towards the centre of the storm, will always cause a certain amount of centripetal motion in the surrounding currents.

48. According to these views, *there is no actual gyration* in the hurricane, but the various currents which contribute to produce it, are first deflected downwards to the earth's surface, and, as soon as they pass its limits, again assume their proper level in the atmosphere, and continue their original course without interruption. It follows, too, from these views, that at a point on the south-western margin of the storm, the raising of the wind from NE. to NW. may be experienced simultaneously, or nearly simultaneously, with the same changes in the centre of the storms; and, also, that strong winds from the NW., veering to SW. and S., may be felt completely to the south-eastward of the point at which the acceleration of the trade-wind first occurred, or

where this was only to a moderate extent. Facts will be hereafter adduced in support of these positions, which are inexplicable on the ordinary theory of rotation.

49. It will now be necessary to point out the circumstances under which these atmospheric currents come to deviate from their ordinary course, and to present the violence of the hurricane, and, also, to explain the gradual fall and subsequent rise of the barometer, and the central calm which is observed during the storm.

50. Hurricanes seem to originate in two ways, which it is necessary to distinguish; in one, the air near the inner margin of the trade-wind is drawn to the southward with great velocity; and the other phenomena of the hurricane gradually supervene; in the other, when the inner margin of the trade-wind is about lat. 12° or 13° N., an indraught of air takes place over the northern part of the South American coast during the day or evening, and the air to the north of it flows towards the point of least pressure, to restore the equilibrium. The velocity this air acquires will always cause it to flow past the point of least pressure, when it will accumulate to the southward until the increased pressure be sufficient to counterbalance the momentum of the current, so that the current from the northward will continue towards this point, only the point itself will clearly be gradually removed to the north. In from 8 to 12 hours, the diminution of pressure would begin to be sensible at Barbadoes.

51. It is obvious, from what has already been stated, that the moon's attraction (35, *et seq.*) on the atmosphere, within the tropics, is quite sufficient, at certain seasons, to increase the velocity of the currents already existing, or even, when exercised in the proper direction, to overcome them at any given point, and to originate others with an opposite tendency. Such changes will, naturally, be much more frequent and abrupt about the inner margin of the trade-wind than elsewhere, though, when local circumstances are favourable, it is not unreasonable to suppose they might occur at any point in its course.

52. If, when the lower portions of the atmosphere about the inner margin of the trade-wind, in August or September,

were either almost quiescent, or moving gently from the NE., the moon passes the meridian in such a declination as to cause a rapid flow of air to any point considerably to the southward, that must of necessity be supplied by the NE. and NW. currents, already so often mentioned, and it is obvious that the NW. current, and the upper portions of the NE., will have acquired a high velocity long before the lower portion of the trade, which is in immediate contact with the earth's surface, and much retarded by its friction thereon, can be materially accelerated. The moment this flow commenced, the absolute quantity of air in a vertical column of the atmosphere, within its limits, would begin to diminish, and, consequently, the barometer to fall, though for hours afterwards, perhaps, there might be nothing in the state of the wind on the earth's surface to excite attention.

53. As the lowest portions of the atmosphere have always a higher temperature within the tropics than those a little higher, the former would gradually rise, while the latter would fall downwards and occupy their place, but, as soon as the ascending portions reached the elevation of the current moving to the southward (52), they would be rapidly carried away by it; and, as the interchange continued, portions of the current itself would be deflected downwards in place of the warmer ascending columns, and, retaining their original velocity, would be experienced below as *strong gusts*, coinciding in direction with the current from which they were derived.

54. A few such gusts, within a short distance of each other, would throw the intervening quiescent air into violent eddies, and destroy the equilibrium which had previously existed among its various portions, and thereby still farther facilitate its ascent in the atmosphere, while its place on the surface of the earth would be occupied by portions of the current, already flowing rapidly over it, rather than by the adjacent portions of the lowest strata, which hitherto have not had their velocity sufficiently increased to flow in readily. The absolute quantity of air in the vertical column of the atmosphere over this point would be thus farther reduced, and the barometer would descend to a corresponding extent.

As the current flowed through the breach thus formed in the lowest stratum of the atmosphere, its velocity could never be sufficiently reduced to admit of its accumulating, so as to restore the equilibrium of pressure, but it would tend to elongate the breach, in a south-westerly direction, the first portions propelling the still air in that direction, and those which follow gliding above them as they became retarded by the resistance they met with.

55. The difference of pressure within the storm, and beyond it to the northward, now becoming very considerable, would cause the lower portions of the comparatively still air around its northern margin to flow towards its centre, still retaining, however, more or less of their easterly tendency. As soon as these became mixed up with the original current, their velocity would be farther increased; they would be swept away, in fact, by the rapidity of its motion.

56. The upper portion of the trade-wind being thus directed downwards, with great velocity, retains its easterly tendency unaltered, but the southerly motion is much greater than usual (2), and the wind, instead of being NE., or ENE., will at first be experienced from NNE. In consequence of this alteration in direction, that part of the trade to the westward of the storm being no longer backed up as before, will, while flowing southward, have its easterly tendency checked, and will gradually flow towards the line of least pressure (as between NW. in fig. 2), being felt as NNE., or N. currents; and thus joining with the one already flowing in the same direction, will gradually acquire the same velocity. The upper part of the trade-wind would obviously be first affected, and subsequently, that below, and *the storm would move slowly to the westward*, the first indication of its approach being a gradual fall of the barometer.

57. While these circumstances are occurring on the northern and western sides of the storm, others of an equally important character must be taking place on the eastern side of its line of least atmospheric pressure. The upper strata of the trade-wind, which were originally moving from the north-eastward, with the same velocity, and in the same direction as that portion which has been deflected down-

wards (54), now meeting with less resistance to the westward, will commence to flow towards the line of least pressure (as at D, fig. 2), their direction becoming at first ENE., a little more to the SE., and still farther to the south the southerly motion will be checked, and they will appear to come from ESE. These portions of air, having already had a rapid motion, and flowing now from a point where they are under a considerably greater pressure than at that to which they move, will have their velocity very much increased, and joining to the current already flowing along the earth's surface, will tend to accelerate it. Here, as on the northern and western sides of the storm, too, the more quiescent lower stratum will, after a little, under the inequality of superincumbent pressure within and around the storm, gradually flow inwards towards its centre (55), so that, at a point completely to the eastward of that at which the storm commenced, a considerable fall of the barometer might occur, without any other remarkable circumstance than a moderate acceleration of the trade-wind, and a slight veering towards the south.

58. That portion of the trade which was to the eastward of the line of least pressure, having thus been gradually drawn from its original north-easterly direction to an easterly one, it is clear that a triangular space to the SE. (A, C, E, Fig. 2), comprised between the original direction and the new one, the apex being at the point of junction of the original and deflected portions of the current (C, Fig. 2), will be in a great measure deprived of its supply of air from the northward. When the difference of pressure over the outer part of this space, and at the point C, becomes sufficiently great, the air will flow towards that point, and the barometer will fall all over it. But here, as elsewhere, the higher portions of the air will first flow towards C, while those on the earth's surface may remain at rest, or retain a slight motion from the NE., until the difference of pressure has become very great.

59. While these changes have been taking place in the lowest current in the atmosphere, others have been simulta-

neously occurring in the NW. current, then next above it.* This, having its velocity increased by the same cause which led to the acceleration of the superior strata of the trade-wind, must have been flowing to the SE. with great rapidity over the storm already raging; but, as the two currents move at right angles to each other, there must always be a bed of air of variable thickness between them (32), which may be subject to gusts in either direction, but which cannot have a determinate motion.

60. As the NW. current flows over the storm, its plane of motion, instead of being nearly parallel to the earth's surface as previously, will become somewhat inclined to it, dipping to the southward. Being thus directed downwards, it will readily penetrate the still or nearly still air, in the triangular space mentioned above (58) to the eastward of the central margin of the trade wind (C A. Fig. 2), and will be experienced as a strong NW. gale, whereon it impinges on the earth's surface. In consequence of the interposed bed of still air between the NE. and NW. currents, after the disappearance of the former, at any point of the storm's course, and before the occurrence of the latter, there will be a short period of calm, liable, however, to heavy gusts from either point, or from the intervening ones.

61. When the NW. current becomes directed downwards, its velocity will be thereby increased; and the latent support being thus withdrawn from those portions of the current to the NE. and SW. of that first affected, they will have a tendency to flow towards the line of least pressure, as is shewn above would occur in the trade-wind (56, 57). Thus, to the NE. of the line of least pressure in this current, the direction of the current would become first NNW., and farther to the east it would even come to N., and the velocity acquired by those portions of air, being in addition to that they previously had to the SE. in common with the remainder of the current, they would, in like manner as in the trade-wind, be productive of the most violent effects, and farther tend to

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* It will be shewn farther on, that, in certain storms in the West Indies the NW. current is not experienced, and that their characters are thereby very much modified.

accelerate the velocity of the air flowing from the NW. which lies immediately to their south.

62. It is also clear that the air to the NE. of the line of least pressure in the NW. current being directed towards that line, within the limits of the storm, instead of flowing on parallel to it, a triangular space will exist in this current similar to that in the trade-wind (58), in which there can be no decided breeze, only, it will lie between the SE. and N. points of the compass, instead of between the SW. and E. points, as in the latter. In the SW., owing to the deficiency of pressure within the storm, the direction of the current will be gradually brought to WNW., and more to the south even to W.

63. As soon as the NW. current begins to be felt on the earth's surface, it will drive off the still air from the space A C B, Fig. 2, and elongate the storm to the SE., and at last, as its force becomes spent, will glide above the lowest stratum of air, which here, owing to the difference of pressure within and around the storm, will have a tendency to flow in the opposite direction, viz., towards its centre.

64. By referring to Fig, 2, it will be seen that the NW. current only partly occupies the triangular space (58) left vacant by the trade-wind, the SW. or returning equatorial current, which is usually found immediately above the NW. one (11), being deprived of its usual support, is deflected downwards, and experienced over the space unoccupied by the lower currents with all its violence. Within the limits of the storm, the air to the westward of the line of least pressure in this current, will flow towards that line, as in the trade-winds and NW. currents, coming to the WSW. and W. Along the east side, too, the air will have its easterly motion diminished, and the wind will back round from SW. to SSW., or S. This equatorial current, as it approaches the southern margin of the deflected portion of the trade-wind (58) extending from D towards C, Fig. 2, will at first tend to drive it back, but towards the eastern margin of the storm, the great inequality of the atmospheric pressure, within and around it, will ultimately cause the air of the *lower stratum* to insinuate itself under the equatorial cur-

rent, which, then gliding above those flowing to the southward, will reassume its ordinary level in the atmosphere.

65. Though it was advisable, for the sake of clearness, to assume that the points of junction with the central edge of each current (47) of the portion of that current which had been drawn towards the line of least pressure, viz. of that from E. with the north-easterly current, of the N. with the north-westerly current, and of the W. with the south-westerly, were situate round a common centre, it does not by any means follow that they are so in reality; in fact, it would appear the point in the trade-wind precedes the others to the westward, and that these follow, varying in relative position according to circumstances.

66. From what has been stated, it will be clear that were an observer so placed that the centre of the storm should pass over him, he would find the barometer begin to fall as the upper portions of the trade-wind, and the NW. current, immediately over him, became accelerated in their course (52); and as the storm moved westward and the central margin (65) of the trade-wind reached him, the fall of the barometer would continue. When the north-easterly current ceased, he would experience a variable period of calm, with occasional severe gusts from NE. or NW. and the intermediate points (60), after which the NW. current would suddenly commence with great violence. As the central margin of the NW. current approached the observer, the barometer would continue to fall, and on its reaching him would obtain its minimum, at which it would remain with calm weather, occasionally interrupted by gusts, between NW. and SW., until the arrival of the central edge of the SW. current, when the wind would suddenly back round to the W. WSW. or SW., and it would gradually rise, until the air along the earth's surface insinuating itself under the eastern part of the south-western current (64), and raising this in the atmosphere, would lead to the re-establishment of the trade-wind under it, and restore the full atmospheric pressure.

67. Thus the fall of the barometer is caused by the gradual removal of the NE. and NW. currents from above the ob-

server, owing to the peculiar deflections they undergo, and which have been described above (57, 62); and the barometer attains its minimum in the interval between the central margins of the NW. and SW. currents, because, there, from the same cause (65), the SW. current is also absent, but no sooner does the latter reach him than the pressure begins to increase and the barometer regains its ordinary elevation. Were the observer anywhere in the triangular space A C E, Fig. 2, he would perceive a fall of the barometer without any thing remarkable in the wind at first, as explained above (58). It is not to be supposed that all these complicated phenomena occur with the regularity here mentioned, but, though subject to variation, the principle remains the same.

68. The immense quantity of rain that falls during a hurricane, and the enormous amount of electricity developed, have led some authors to suppose that either, or both of these, were mainly instrumental in producing the storm, instead of being effects merely, of the disturbance of the atmospheric currents when nearly saturated with vapour.

69. The dew-point is usually high during the hurricane months in the West Indies, and when the free movement of the atmospheric currents is checked, it often attains the height of 75° or 76° Fahr., while the temperature of the air at the same time may not exceed 78° or 80°. Under such circumstances, the sky, towards noon, often becomes overcast, and, I think, I never saw, even in winter, in this country, more gloomy and dismal weather than may not unfrequently be seen at Barbadoes at mid-day, in August or September. The immense mass of vapour necessary to obscure so completely the rays of a vertical sun, could only be produced in a bed of the atmosphere of great thickness, saturated with moisture. That much rain falls under such circumstances is not astonishing, but their frequent occurrence, in the localities most subject to hurricanes, without producing any upward current, as maintained by Mr Espy, is certainly strong evidence against the correctness of his theory.

70. The justness of these views is strikingly illustrated by

the account of the Barbadoes hurricane of 1831, given by Colonel Reid in his work; and, considering the importance of the subject, I trust I shall be excused for introducing it here with little abbreviation. This hurricane seems to have originated in the immediate neighbourhood of Barbadoes, and the state of the weather during the day preceding its occurrence, has been noted with great minuteness.

"On the 10th August 1831, the sun rose without a cloud, and shone resplendently. At 10 A. M., a gentle breeze which had been blowing died away. After a temporary calm, high winds sprung up from the ENE., which in their turn subsided. For the most part calms prevailed, interrupted by occasional sudden puffs from between the north and north-east.

"At noon the heat increased to 87°, and at 2 P. M. to 88°, at which time the weather was uncommouly sultry and oppressive.

At 4 the thermometer sank again to 86°. At 5 the clouds seemed gathering densely from the north, the wind commencing to blow freshly from that point; then a shower of rain fell, followed by a sudden stillness; but there was a dismal blackness all round. Towards the zenith there was an obscure circle of imperfect light, subtending about 35 or 40 degrees.

"From 6 to 7 the weather was fair, and wind moderate, with occasional slight puffs from the north, the lower and principal stratum of clouds passing fleetly towards the south, the higher strata or scud flying rapidly to various points.

"At 7 the sky was clear and the air calm; tranquillity reigned until a little after 9, when the wind again blew from the north.

"At half past 9 it finished, and moderate showers of rain fell at intervals for the next hour.

"Distant lightning was observed at half past 10 in the north-east and north-west. Squalls of wind and rain from the NNE., with intermediate calms succeeding each other until midnight. The thermometer meantime varied with remarkable activity; during the calms it rose as high as 86°, and at other times fluctuated from 83° to 85°. It is necessary to be thus explanatory, for the time the storm commenced, and the manner of its approach, varied considerably in different situations. Some houses were actually levelled to the earth, when the residents of others, scarcely a mile apart, were not sensible that the weather was unusually boisterous.

" After midnight, the continued flashing of the lightning was awfully grand, and a gale blew fiercely from the north and north-east; but at 1 A. M., on the 11th August, the tempestuous rage of the wind increased; the storm, which at one time blew from the north-east, suddenly shifted from that quarter, and burst from the north-west *and intermediate points*. The upper regions were from this time illuminated by incessant lightnings; but the quivering sheet of blaze was surpassed in brilliancy by the darts of electric fire which were exploded in every direction. A little after 2, the astounding roar of the hurricane, which rushed from the north-north-west and north-west cannot be described by language. About 3 the wind occasionally abated, but intervening gusts proceeded from the south-west, the west, and west-north-west, with accumulated fury."

At this time a number of meteors fell, and a few moments after the " deafening noise of the wind sunk to a solemn murmur, or, more correctly expressed, a distant roar," and the lightning, " for the space of nearly half a minute, played frightfully between the clouds and the earth with novel and surprising action."

" The moment after this singular alternation of lightning, the hurricane again burst from the western points with violence, prodigious beyond description, hurling before it thousands of missiles—the fragments of every unsheltered structure of human art. The strongest houses were caused to vibrate to their foundations, and the surface of the very earth trembled as the destroyer raged over it.

" After 5 o'clock the storm, now and then for a few moments abating, made clearly audible the falling of tiles and building materials, *which by the last gust had probably been carried to a lofty height.*

" At 6 A. M. the wind was at south, and at 7 south-east; at 8 east-south-east, and at 9 there was again clear weather.

"From the direction in which the cocoa-nut and other trees were prostrated next to the earth, the first that fell must have been blown down by a north-north-east wind; but far the greater number were rooted up by a blast from the north-west." *

71. The state of the barometer during the hurricane, according to Lieutenant Bynn, fort-adjutant at Barbadoes, was on

August 10th at 8 P. M. 30·00 in.
11th ··· 2 A. M. 29·40.
··· ··· 4 ··· supposed to be under 28·00.†

These, however, lose much of their interest from the absence of the mean height of the barometer, and of any notice as to the nature of the particular instrument.

72. On the 10th August 1831, the sun's declination, on the superior meridian at Barbadoes, was 15° 41′ N.; the moon's declination on the same 5° 7′ N., and her hour of transit 2^h 27^m P. M. Now, according to the principles noticed in the first part of this communication, the sun and moon passing the inferior meridian, with a northern declination, would accelerate the trade-wind, while they checked the upper current from the SW.; and, as the sun approached the superior meridian, his declination being greater than the latitude of the place (Barbadoes lies between 13° and 13½° N.), as soon as his heat began to affect the air decidedly, that would have a tendency to rise from the earth's surface at a more northerly point, and there join the currents returning to the northward, and, consequently, all that portion of the trade which was to the southward of this point would either simply have its southerly motion gradually stopped, or it might have it more or less reversed, and flow back from the ESE. SE. or even S. Again, the moon passing the superior meridian with a declination of 5° 7′ N. only about 2½ hours after the sun, would cause a flow of air to those points through the zenith of which she passed, and tend to re-establish the trade-wind (19, 32, 50); but it is obvious that

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* Reid on the Law of Storms, 2d edit., &c., p. 29–32.

† *Ibid. p. 37.*

the upper portion of the trade, and the NW. current, would have acquired a great velocity long before the air in contact with the ground became sensibly affected.

73. Referring, now, to the description of the hurricane given above, it will be immediately remarked, that on the morning of the 10th August 1831, the weather was remarkably fine, with a gentle breeze, which died away by 10 A.M. From 10 to mid-day calms prevailed, with occasional flaws, such as are usual in that climate, towards noon, when the air is still. At noon the thermometer was 87°, and at 2 P.M. 88°, with "uncommonly sultry and oppressive" weather, still indicating the persistence of calm in the lower regions of the atmosphere. The temperature fell to 86° at 4 P.M. From 5 to 6 the clouds seemed gathering densely from the north, and the sky became generally overcast, while, beneath, the calm continued, though interrupted occasionally by flaws.

74. "From 6 to 7 the weather was fair, and the wind moderate, with occasional slight puffs from the *north; the lower and principal stratum of clouds passing fleetly towards the south, higher strata or scud flying rapidly to various points.*" By 6 P. M. the tide in the atmosphere, produced by the moon's attraction, (72), had had about 3½ hours to become developed, and the currents thereby induced are indicated in the above passage with a clearness that admits of no doubt.

75. From 7 to 9 the sky was clear and the air calm. From 9½ to 10½ P. M., the wind was *north,* with slight showers. From 10½ P.M. until midnight, there were "*squalls of wind and rain from the north-north-east, with intermediate calms succeeding each other;*" the thermometer during the calms stood as high as 86°, and at other times varied from 83° to 85°. During these squalls, houses were levelled, while a mile off the weather was not considered unusually boisterous. Here it is to be observed that the wind first came from the *north* (56); that *heavy gusts* were afterwards felt from *north-north-east,* during which the thermometer fell considerably, thereby shewing that the air forming these gusts came from a considerable elevation; and also, that previous

to its deflection downwards, it must have been moving from NNE. with great velocity (53, 74).

76. "From midnight to 1 A. M. on the 11th August, the 'gale blew fiercely,' and at 1 seems to have drawn round as far as *north-east*, a change that would necessarily occur as the gale proceeded westward (57). About this time the force of the wind increased, it *suddenly shifted from the north-east, and burst from the north-west and intermediate points.*" This gradual veering from the south to the north-east, according to the rotatory theory, would have placed the centre of the storm to the southward of the island, and, according to it, too, the subsequent backing from north-east to north-west, under these circumstances, should not have occurred. If the centre of the storm passed to the northward of the island, the wind, according to the rotatory theory, should have *backed gradually* from north-east to NNW. and NW., and not '*suddenly*' from NE. to NW., as was actually observed. The gradual veering from N. to NE., according to the centripetal theory, would have placed the centre of the storm to the south of the island, and, were it correct, the wind should have veered to the eastward, and have gradually ceased. On the contrary, these changes of direction may not only be completely explained on the supposition of two currents flowing from the NE. and NW., with an intermediate stratum of air (32, 59), but in their turn afford the most convincing proof that such actually exists.

77. After 2 A.M. the wind was *north north-west*, and *north-west.* About 3 it *occasionally abated*, "*but intervening gusts proceeded from the south-west, the west, and west north-west, with accumulated fury.*" According to the centripetal theory, a storm in which such changes occurred should have been proceeding northward, yet the course of this hurricane was most satisfactorily shewn by Colonel Reid to be westerly. It would be very difficult, too, to account for all these changes of wind on the principles of pure rotation, though by referring them to the different currents which usually prevail in the atmosphere within the limits of the trades, every shift, every squall even, may be satisfactorily explained (32, 59).

78. About 3 A.M. there was *a temporary lull* accompanied

by a remarkable display of meteors, immediately after which *the hurricane again burst from the western point with violence prodigious beyond description.* Here, again, the stratum of air lying between the NW. and SW. currents, which had been subject to gusts from SW. to WNW., had no sooner passed to the westward than the south-westerly current *suddenly impinged* on the ground with the utmost violence.

79. After 5 A.M. the wind began to abate in force, and the excess of atmospheric pressure around the storm which had previously been counterbalanced by the violence of the SW. current, now began to act by causing the air to flow in from the south and east (64), so that at 6 A.M. the wind was south, at 7 A.M. south-east, at 8 A.M. east-south-east, and at 9 clear weather.

80. This storm was experienced at St Lucia, St Vincent, and in the neighbourhood of Grenada. The northern extremity of the first island is about 94 geographical miles NW. by W. of the centre of Barbadoes, the east coast of St Vincent about 86 miles W. of Bardadoes, and the north point of Grenada is nearly SW. by W. of Barbadoes 130 miles. Now, supposing the hurricane to have been strictly rotatory, and its centre to have passed a little to the north of Barbadoes, its course being, according to Redfield's chart (No. 3 in Reid's work), WNW., it should have commenced, first at St Lucia, then at St Vincent, and afterwards at Grenada, the direction of the wind at each being a tangent to the circle of rotation. But it first commenced near Grenada, where the army Schooner, Duke of York, began to experience *heavy squalls from the north-west about midnight*,* fully an hour before the wind at Barbadoes backed from NE. to NW., and while there was nothing in the state of the weather at St Lucia or St Vincent, to excite attention. Towards the north of St Lucia it began to blow hard at 4 A.M. on the 11th, from the *north*,† and at St Vincent the hurricane commenced at 7 A.M. from the *north-westward*.‡ These facts, which are utterly inexplicable on the principles of simple rotation, are readily and naturally accounted for by

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* Reid, p. 33.

† Ibid. p. 32.

‡ Ibid. p. 27 and 33.

the existence of the trade-wind and NW. current in the atmosphere, the latter impinging on the earth's surface at points over which the former did not extend (47, 60).

81. At the northern extremity of St Lucia, the force of the hurricane was greatest between 8 and 10 A.M., from which time the wind veered round to east, and died away at south-east. At the southern extremity of the island, the most violent part of the storm was from the south-west; and in St Vincent, which lies next to the south of St Lucia, the wind backed from NW. to W. and SW.*

82. If the directions of the wind at St Lucia, St Vincent, and Barbadoes, at any given hour, be compared, it will be found that they do not agree with the rotatory theory. At 7 A.M. on the 11th August, for instance, the wind at St Lucia was NE., at St Vincent NW., and at Barbadoes SE. Now, according to the rotatory theory, at the latter the wind should have been about SW. by S., shewing a difference of seven points, or 79° between the theoretical and true directions.

83. In the accompanying diagram (fig. 3), Barbadoes, St Lucia, and St Vincent, are laid down, and the direction of the wind throughout the storm, supposing it to be composed of different currents, as advocated in this paper. I have assumed that the centre of the hurricane was a little more to the south than Reid supposed, and that its tract, instead of being WNW., as laid down by Redfield, was at first not higher than W. by N. ½ N. The north-westerly wind at Barbadoes, came "from the north-north-west and north west" (70), it is projected in the diagram as NW. by N., the intermediate point. The thick line shews the course of the centre C of the storm, and its position is indicated for every hour the dotted lines, parallel to it, pass through Castries in St Lucia, and another point in the south of the island, through Bridgetown in Barbadoes, and Kingston in St Vincent, and by following either of these from west to east, the changes of direction of the wind at each point, as the gale passed over it, will be readily seen. These will be

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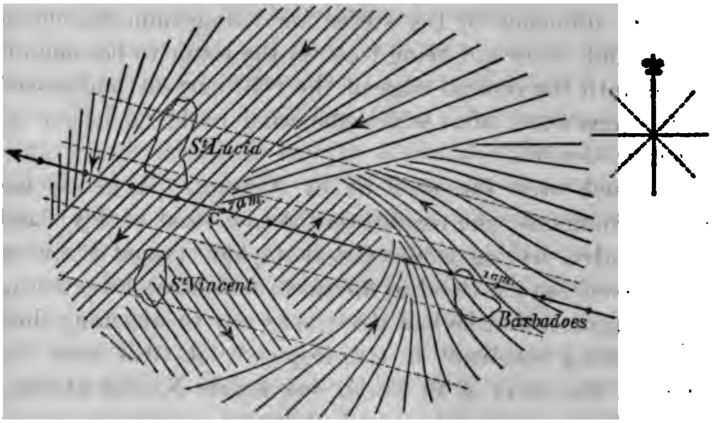
* Reid, p. 32-3.

found to agree much more closely with the above statements, not only as to direction, but also as to the time of their occurrence, than any laid down on the principles of true rotation.*

Fig. 3, shewing the directions of the wind during the Barbadoes hurricane of 1831, on 11th August, at 7 A.M.



84. A gentleman named Giltens, residing at Barbadoes, prognosticated the occurrence of this hurricane at 4 P.M. on the 10th. The chief indication he relied on seems to have been "the darting forward of the clouds with flat irregular motion, not borne by the wind, but driven as it were before it." †

85. It may be asked how, if the first phenomena of a hurricane consist merely of an acceleration of the trade-winds, this should not have been experienced in the above instance at St Vincent, as well as at St Lucia. There is, unfortunately, no account of the state of the weather at St Vincent previous to the commencement of the hurricane, but from collateral evidence the force of the wind does not seem to have been unusual. The distance from the northern extremity of St Lucia to the middle of St Vincent is about 60 miles, and though the hurricane might have been raging with great violence from NNE. or NE. at the former place, still as the

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* If the lines indicating the direction of the wind be taken off on tracing paper, and this moved along the track of the storm so as to bring the point C over its place for any given hour, the direction of the wind, at that hour, will be immediately seen at every point within its range.

† Reid, p. 36.

wind passed south, the first portions of air would meet with others moving slower, which would impede their onward progress, and afterwards, the lower portions having their motion checked by friction on the earth's surface, those which followed would at length glide over them (54), and thus form a strong NE. current at some elevation ; that being protected from its influence by the stiller air resting immediately on the ground, there not being time for the latter to become affected until the central edge of the NE. current had moved to the westward, after which the wind could no longer be felt from the NE.

86. Such was the case at St Vincent. A gentleman named Simmons, who resided on the east coast of this island at its centre, having ridden out at daylight (about 5 A.M. at this season), saw a cloud to the north of him, so threatening in appearance, that he had never seen any so alarming during his long residence in the tropics.* At this hour the wind, at the north of St Lucia, was nearly N. and blowing hard. When the hurricane did commence at St Vincent, it had nearly attained its greatest force at St Lucia, but was still north-easterly, so that though the storm was raging at no great distance to the north, it did not appear at St Vincent for several hours after, and not until there was sufficient time for the central margin of the NE. current to have moved to the westward, so as to permit that from the NW. being directed downwards (60).

On the Beneficent Distribution of the Sense of Pain. By Mr G. A. ROWELL. Communicated by the Author.†

Having had, in my youth, an aversion to animal food, from an idea that it was cruel to destroy life for the purpose of obtaining it, I have been led by that feeling, and a few rather extraordinary circumstances which have come under my notice, to pay some attention to the effects produced by injuries to various animals ; which investigation has caused such a complete change in my opinions on the use and distribution of the sense of pain, that, so far from considering it an infliction, I now believe it to be one of the most necessary senses we possess; that, like all other senses, it is given to animals in as great a degree as it is necessary and useful to them; that no animals have a greater sense of pain than is necessary for the preservation of the class to which they belong; that those which are designed for food, suffer little when killed, in comparison to what other animals would feel from the same infliction; and that some are totally devoid of the sense of pain.

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* Reid, p. 27.

† Read before the Ashmolean Society of Oxford, 3d May 1847.

In submitting this paper to the consideration of the Ashmolean Society, I beg to state distinctly, that I do not pretend to any knowledge of anatomy, but have been led to my conclusions by what appears to be the effect of injuries to different animals. I do not attempt to assign any cause for the difference of the amount of pain, whether it be that the nerves are less sensitive, or less numerous in some classes than in others, or whether it is owing to the want of reflecting faculties, but only to shew that there is such a difference.

I do not know that there is any thing new in the opinions I advance; but as I have had more than ordinary opportunities of witnessing the effect of wounds on some classes of animals, I submit this paper, believing that the consideration of the subject is calculated, in the highest degree, to excite feelings of gratitude and admiration of the merciful designs of Providence; and as the discovery of the use of the vapour of ether has recently brought the sense of pain under the consideration of this Society, I hope the paper will be in some degree interesting.

There can be little doubt that the sense of pain is of the first importance to man, to guard and warn him from injury. The skin is very sensitive, the body being thus enveloped in a membrane susceptible of the slightest injury, while the heart, lungs, brain, and other vital internal parts that are thus guarded, are almost insensible; but although the lungs are, in a great degree, insensible of pain during consumption, they are extremely sensible of the impurities of the air, thus guarding against the inhalation of anything injurious. As the hands, and especially the fingers, are very liable to injury, the sense of pain is great in those parts; and I believe there may be more real pain from a gathering in the finger, than from very many of the most fatal complaints. The exterior coating of the eye is extremely sensitive; while the back and interior portions of that organ are almost insensible. The sense of pain in the mouth guards the throat, and in the stomach is a warning against our eating anything that is injurious. Rheumatic pains are bad; but how many more fatal cases would arise from colds, &c., if man was not warned by pain and inconvenience of the bad effects upon his constitution of sudden changes of temperature.

One of the best ways to judge of the value of anything, is to consider how we could do without it; and it will be well to do so in the present case. Thus, if a man had not the sense of pain, he might sit by a fire, and, in his absence of mind, put his foot upon it, and soon find himself *minus* that useful member; he might have lime blown into his eyes, and thus lose his sight if not warned by the pain; in fact, there would be no end to his dangers if not possessed of that useful monitor, which guards him from injury, and is a check to his excesses. There may be pains and sufferings, the use of which it may be difficult to see; but I would rather attribute this to a want of knowledge, than believe that the rule which holds good in so many cases does not hold good in all. In fact, the beneficial use of the sense of pain to man is so evident, and has been pointed out so long since in Paley's Natural Theology, that I should not have said anything upon the point, but that I considered it necessary for the elucidation of my subject, as regards its uses and distribution amongst the lower classes of animals.

Before I enter farther upon the subject, it will be necessary to consider what may be taken as a *proof* of pain; convulsions are considered by many as a sign of suffering, but I believe it is generally allowed by the medical profession that that opinion is erroneous: the cry of animals cannot always be depended on as indicative of pain, which is proved by the noise a pig will make when taken hold of. It is also necessary to make allowance for the struggling under restraint which is natural to all wild animals. The only criterion to decide the question is to consider what is the effect of mutilations on the health of animals, and how far such injuries interfere with their usual habits and appetites.

I will now state a few cases, to shew that injuries, apparently the most dreadful, have but little effect on many of the brute creation.

The first case which forcibly took my attention, was seeing a horse that was feeding by the side of the road between St Clement's and Headington hill, have its leg broken by a coach-wheel passing over it just above the fetlock joint; the poor beast shewed evident signs of pain at the moment, the bone being dreadfully crushed, and protruding in parts through the skin. A number of persons collected around, but no one liked to despatch it, and on their standing aside, so that it might get out of the way of things passing, the moment the horse got to the side of the road it began grazing, shewing no other sign of pain than holding up the injured leg.

Another case is that of a post-horse, which was going along the road between Botley and Ensham, about twelve years since, when it came down with such violence that the skin and sinews of both the fore fetlock joints were so cut that on it getting up again the bones came through the skin, and the two feet turned up at the back of the legs, the horse walking upon the ends of the leg bones. The man who was with it would not consent to its being killed till he had informed his master (who, I believe, was Mr Masters of Staple Hall Inn, Witney); the horse was therefore put into a field by the road side, and was found the next morning quietly feeding about the field with the feet and skin forced nearly half-way up the leg bones, and where it had been walking about, the holes made in the ground by the leg bones were 3 or 4 inches deep.

A similar accident once happened to a coach-horse, the property of the late Mr Costar of Oxford; it was found, when the coach stopped to change horses, to have dislocated the fetlock joints, and from the worn appearance of the ends of the leg bones, must have run a considerable distance along the road in that state.

I do not lay much stress on this case, as it is not very surprising that a spirited horse, in harness with others, should continue running under such circumstances; but, in the former case, there was nothing to excite the horse but its hunger, and if the pain had been equal to what such a dreadful injury would seem to indicate, it would probably, if in ever such a famished state, have gone upon its knees to feed, rather than upon the injured parts.

It is curious to observe the apparent indifference with which some animals will devour parts of their own bodies. I once kept tame dormice, and, in shutting the cage-door, accidentally caught the tail of one of them, when it squeaked out and left the skin of about two-thirds of its tail sticking to the door. Whether the cry was caused by pain or fear, I cannot decide; but it went about the cage for a few minutes apparently rather uneasy, it then took hold of its tail with its paws and eat all the injured part, and then seemed as well as ever.

Rats will often eat their tails when in confinement, if kept short of food; and the habit of eating their own tails is not uncommon amongst the monkey tribe. I know a person who used to dip the end of his monkey's tail in tobacco water to keep it from being eaten, and some of the monkeys in the London Zoological Garden may at times be seen enjoying themselves in this way; but from whatever cause this propensity may arise, I believe it is never indulged in by the monkeys with prehensile tails; their tails seem to be too useful to be so wantonly disposed of, and I have no doubt are therefore possessed of a much greater share of the sense of pain.

A few years since, the Quarterly Review, in a notice of the Dean of Westminster's Work on the bones found in the cave at Kirkdale, stated that an old hyena kept in the Jardin des Plantes at Paris, had its leg broken, when one night it bit off the leg at the broken part, and eat it.

The emasculation of large cattle seems a very barbarous operation, the parts being cut with hot instruments; yet I saw an aged bull after undergoing that operation, walk away very unconcernedly, and then after grazing for about half an hour, he lay down and chewed his cud apparently quite comfortable.

Pigs make a sad outcry when being killed, but I believe it is caused by fear and the uncomfortable way in which they are held, rather than by pain. I once saw a large pig which had been stuck, get away from the men who were holding it, and there was not the least cry after it had got out of their hands, although it was bleeding to death: when smaller pigs are killed by sticking them, and then letting them run about till they drop, there is no cry after they are let go; and if stuck skilfully, without taking hold of them, there is no more noise than a mere grunt or squeak, about the same as there would be if the pig had a slight blow with the end of a stick; and I have no doubt that a pig may feel more pain from a heavy blow, than from being killed in the usual manner. When it is considered that the nose of a pig is so very useful to the animal from its habit of routing in the earth, and may therefore, be very sensitive, it does seem probable that the opinion is correct, that a pig feels more pain from having a ring put through its nose, than in being killed.

I have stated these cases to shew that the pain felt by brutes is much less than would be felt by man under similar injuries. My object is to shew the probability, that as the sense of pain is not so necessary or useful to brutes, they have it in a less degree.

In the next class of animals to which I shall allude, that is, rabbits and hares, I will endeavour to shew that the use of the sense of pain is, in a great degree, or almost completely, superseded by other senses, and that their sense of pain is very trifling, compared to that of most other quadrupeds. There can be little doubt that, although so very prolific, very few rabbits or hares in a wild state die of old age, as they are the food of a large class of beasts of prey. Foxes, wild-cats, martins, pole-cats, stoats, and weasels, could not exist without them; they are their natural prey, against the least of which the rabbit or hare has no means of defence when once caught; therefore, the sense of pain would be of no use to them, either to warn them from danger or to cause them to exert themselves to escape; but a slight examination of the form of both rabbits and hares will shew that they *have other* means of defence: their eyes are not placed in the front of the head as in beasts of prey, but on the side of the head, very prominent, so that they are enabled to see before, behind, and all around them; their ears also can be turned this way or that way to catch the slightest sound, added to which, they have a degree of timidity which keeps them always on the alert.

With regard to their sense of pain, it is well known that a hare never, or very seldom, cries out when shot, even if she receives her death-wound, if she can run a few yards and hide herself; but if her legs are broken, or she is in any way stopped from running, even if caught in a net, which can give her no real pain, she utters most piteous screams; when followed by dogs, her screams always begin before they have actually caught her, and it is worthy of notice that she is much more readily despatched than perhaps any other animal of her size.

Rabbits resemble hares in this respect, as they utter no cry when wounded, but will do so from fear: if run down by a stoat or weasel, they always cry out when the enemy gets within two or three yards of them, and are generally so terrified that they lie down and are caught; therefore the cry in this case is evidently from fear.

I one day disturbed a rabbit which ran away in so singular a manner that I followed it, and saw that the flesh had been eaten away from the back of the head to the top of the shoulders; the sight was so sickening, that I turned away, thinking the poor creature could not live many minutes. About two hours afterwards, I went with a view to pick up and examine the rabbit, and when I came to the spot, was surprised to see it jump up and run away as before; the person who was with me ran after it, the rabbit ran into a bush, and he caught it. Now, although the poor thing was so injured, there was no cry when I first disturbed and ran after it, or when it was disturbed and followed the second time, but the moment it was caught it began to cry out, shewing that fear could excite a cry which all its sufferings could not do.

When rabbits are caught in traps, if not taken out in a short time they are almost sure to escape, either by breaking away by force from the trap, and tearing off the leg, or by biting the leg off. These traps are made to clasp very strongly, but, to prevent escape as far as possible, they are made purposely not to close nearer than about the eighth of an inch, and the teeth are rounded so as not to cut; but rabbits are so indifferent to pain that I have seen their legs left in traps with the sinews attached to them, just as the sinews are drawn from a fowl's legs previous to cooking, and yet although the bone is so broken, and the muscles and sinews torn apart in this manner, it seems to have little or no effect on the health of the animal.

I have seen them caught after having recently lost a leg, and to all appearance in as good health, and as fat, as if nothing had happened to them. A short time since, I saw a rabbit caught which had but one leg, having lost one hind-leg apparently some time, and the two fore-legs very recently, but although the poor animal had been obliged to go along as it could with its one leg and the bare stumps of the others, it was in good condition and healthy.

Rats will bite off their legs in a similar way, and escape; but I do not know of any animal which is strictly a beast of prey, or rather a hunting animal, that will do so. I have never known a cat, polecat, or such animals do it, although they may sometimes lose a leg in a common trap which shuts close and is apt to cut; and I have lately known a fox found in a wood in a dying state, from starvation, with a trap on its leg, an incumbrance that a rabbit would have been free from in a very short time.

These facts, will, I believe, bear me out on the point, that the sense of pain is for the preservation of animals, by compelling them to take due care of themselves, and that no animal has a greater share of the sense of pain than is necessary for the preservation of the class to which it belongs. The loss of a leg must be a great inconvenience to any quadruped, but rabbits or rats may still procure food without it; even the case before alluded to of the hyena, does not tell against it, as the hyena does not get its food by swiftness of foot alone, nor is its foot the weapon of attack, as with the *cat tribe*; but if a fox, wild-cat, polecat, or any animal of that description, loses its leg, it is a great chance if it does not die of starvation, unless its prey be very plentiful; therefore, as the legs are of so much importance to these animals, they seem to be endowed with a sense of pain in proportion to their usefulness, as a guard for their preservation.

From the ease and indifference with which crabs and lobsters will throw off their claws, when under the influence of sudden fright, or when their claws are injured, it is evident that their sense of pain from mutilation must be very little; and, according to the argument I have advanced, it may be asked, of what use can the sense of pain be to any of the crustaceous tribes? They are coated in armour sufficient to protect them from all minor enemies, and if they get into the power of an enemy strong enough to crush through their shells, of what use can the sense of pain be to them then? But they shew signs of great pain when thrown into boiling water, and I see no reason why they may not be insensible to pain from mutilations, and yet be sensible, in a high degree, to pain from extreme changes of temperature; for, as there are no bounds to prevent their ranging over all parts of the sea, it is probable that a sense of temperature is necessary to them, and many other animals also, to keep them to those parts of the world which they are formed to inhabit. It may also be useful to keep them to those parts and depths in the sea that are necessary for their young to arrive at maturity.

From the observation of Sir Humphry Davy, and others well acquainted with the habits of fish, it is very probable that the sense of pain in many of them is very trifling; and when we consider the fecundity of the cod, and many other fishes, the number of eggs deposited by a single cod-fish in one season (according to Leuenhock, upwards of nine millions), and bear in mind, that unless these fish more than double their numbers annually, that of the myriads of fishes that are hatched from these eggs, not more than three or four, on an average, can arrive at the same maturity as the parent fish, the others being all destroyed at different stages of their growth, being the food of other animals; it does seem probable that there is a similar dispensation of mercy to them, as I have endeavoured to shew there is to rabbits and hares.

Many cases may be brought forward to shew the absence of pain in insects. I have seen a wasp eat a fly almost immediately after a portion of its own abdomen had been cut off; I have also seen a cockchafer crawling and eating on a hedge after its abdomen had been emptied of the viscera, probably by some bird. It is well known that a dragon-fly will eat freely for a considerable time while confined by a pin through its body; and every one who has collected entomological specimens, must know the difficulty in killing some of the larger moths. But as this paper is already much longer than I originally intended, I shall say no more on this point, as the remarks on the subject in the introduction to Kirby and Spence's Entomology are very generally known.

It may be well to reflect for a moment on the constant slaughter that is going on amongst the lower class of animals. The number of flies eaten by a single pair of swallows and their young must be immense.

I once observed a rather extraordinary illustration of the law of nature to eat and be eaten. I kept in a glass globe a variety of the smaller aquatic animals, such as the larvæ of the dragon-fly, &c., and one day introduced amongst them a few of the common water newts and water beetles, one of which was the ditiscus marginalis. The dragon-flies had been living upon the animalculæ, &c.; the newts attacked and devoured the dragon-flies. The next morning I found one of the newts lying at the bottom of the vessel half-eaten, and, while looking on, saw the ditiscus attack another newt. Not wishing to have them all destroyed, I took the ditiscus out of the water, and put it in the sunshine a few minutes, when it flew away, and had not gone more than 30 or 40 yards when a sparrow flew after and caught it. This constant destruction of life would be fearful to contemplate, if there is truth in the quotation so often made, that "the poor beetle that we tread upon, in corporal sufferance finds a pang as great as when a giant dies."

It may be objected to what I have advanced, that where there are nerves, there must be a sense of pain; but I see no reason why the nerves may not differ as much as the animals to which they belong; and the use of the vapour of ether shews that nerves may exist without the sense of pain; for, if I understand the subject rightly, the patient, if not completely insensible, may be sensible of feeling while insensible to pain.

It may also be objected that I can bring no *proof* of brutes not feeling pain in the same degree as man, but only that they do not shew *signs* of so much suffering. To this I would answer, that while I admit that man has no right wantonly to destroy or mutilate any animal, yet, in searching into the works of creation, seeing that it is a law of nature that some animals *must* be destroyed that others may exist, we have no right to assume, that in this destruction any animal feels more pain than it shews signs of.

G. A. ROWELL.

April 19, 1847.

SCIENTIFIC INTELLIGENCE.

GEOLOGY.

1. *On the Accessory Elements of Pyrogenous Rocks, by M. J. Durocher.*—Independently of the silicated minerals which constitute the crystalline rocks called pyrogenous, these rocks contain accessory elements, found in small quantities it is true, but which must be taken into account in the study of geological phenomena. M. Durocher's researches on this subject have led him to the following results:—

Almost all the pyrogenous rocks contain water in combination which is not disengaged but between 100° and a dull red heat, *le rouge sombre*. They contain it in proportions very different, and variable between certain limits; the ordinary granites, such as have undergone no apparent alteration, contain from 1 to 5 thousandth parts. When a granite has begun to alter by losing its lustre and hardness, the quantity of water found in it increases rapidly to 0·03 and 0·04; besides, even in granites which appear unchanged, the portion consisting of felspar habitually contains a little water, although less than the mica. Petrosilex and the quartziferous porphyries contain water like the granites, and so much the more the greater the alteration they have undergone. In diorites which have undergone no alteration, M. Durocher has found the proportions of water to vary from 0·008 to 0·020. The majority of trap rocks contain from 0·020 to 0·060. In the pyroxenic porphyries, basalts, and trachytes, there is usually, with rare exceptions, from 0·007 to 0·030, and in the pumices there is much more, from 0·030 to 0·070. In six volcanic lavas he found from 0·0045 to 0·0451 of water.

M. Durocher has ascertained the existence of carbonate of lime and dolomite in many pyrogenous rocks when it is impossible to perceive them by means of a magnifying glass; he has discovered this by treating small portions of these substances with acetic acid. He has likewise found quantities of dolomite, varying from 2 to 13 thousandth parts in a granite from Stockholm, in a protogene of the valley of Agly (Eastern Pyrenees), a petrosilex of Sala (Sweden), a euphodite from Savoy, a basalt of St Flour, and in a vesicular and peridotiferous lava from Auvergne. He has detected traces of carbonate of magnesia in a hyperstenic syenite from Norway. He has also ascertained the presence of from 0·001 to 0·016 of carbonate of lime, either pure or slightly magnesiferous, invisible with a glass, in 25 specimens of granitic, amphibolic, trap, basaltic, pyroxenic, and trachytic rocks, as well as in lavas from different countries.

Thus, independently of the silicates, the pyrogenous rocks contain a little water and very often small quantities of earthy carbonates, which appear to have formed part of them from the first, and not to have resulted from infiltration. Frequently also they contain, as is already known, small quantities of phosphates, *fluorides*, sulphurets, and arseneosulphurets.

The magnetic property is much more common in these rocks than is generally supposed. The granites alone are rarely magnetic. The magnetism of these rocks appears to the author to be owing to the presence of a small quantity of oxidulated iron, sometimes in titaniferous iron or magnetic pyrites. In other respects it is remarkable that the greater part of crystalline rocks, even those which are not magnetic, yield a little oxide of iron to boiling acetic acid; when they are calcined, they almost always assume a reddish or rose tint.

M. Durocher concludes by adding that the Oligoclase, that species of felspar which was first observed in the granitic rocks of the north of Europe, is likewise found, though in smaller quantity, in those of France. He has discovered it in a syenite of the Vosges, and in numerous granites of the Alps, Pyrenees, and the west of France.—*L'Institut*, No. 709, p. 249.

2. *Baron Von Buch on Glaciers.*—I think I may now well venture on this conclusion, deduced from nature and experience, without needing to fear uncalled for verbose, and therefore unmeaning, opposition, since the singular doctrines of Venetz and Charpentier, re-

garding the origin and progress of glaciers, are only heard in the far distance, and since the general conviction that Agassiz's unsuccessful attempt to live three summers on a glacier, and all the care and labour expended there, have led to no other result, than to confirm still more the wise considerations and deductions of Saussure, and to prove that the faculty of extended generalization, which depends on few but sure observations, leads sooner and more directly to the truth than all the instruments we may heap together, without using them with proper precautions. Even the echo, still faintly repeated from the other side of the Atlantic, will in a short time cease. When we question the maps of the Swiss Alps, the mountains in the Tyrol, the Norwegian glaciers, the few seen in the Pyrenees, the magnificent vicinity of the sources of the Ganges and Jumna,—everywhere the same law appears, namely, "That glaciers only form on mountains that rise above the limits of perpetual snow, and spread out in this region; the origin of such glaciers must be sought in depressions,—wide basins of snow. They never originate on the open rocks far from large masses of snow. From these wide pots of snow, the icy mass proceeds down in deep valleys, perhaps even to inhabited places, where the temperature of the air sets limits to its farther progress, and where the portion destroyed by melting must be continually and rapidly renewed from above." From these essential conditions in the formation of glaciers it evidently results, *that the cause of their progress and sliding down into the valleys must be sought, either altogether, or at least chiefly towards their origin, and above the snow-line*, never in the ice masses themselves, which in this respect are altogether passive. In this upper region the pressure of the connected ice masses operates, exactly as the pressure on the Rossberg has pushed down a whole stratum of the mountain, destroyed it, and covered half a canton with the giant fragments; and this pressure is not destroyed in its progress, but increased until the temperature and smaller declination of the valley are able to counterbalance the pressure of the mass. No glacier continues to move when the bottom of the valley on which it rests has a less inclination than 3° (Elie de Beaumont.) No doubt large extended masses of snow often appear in confined valleys below the snow line; they may be even changed into vaults of ice as is so beautifully seen in the ice-chapel not far from the Bartholomæus lake at Berchtesgad; only these never move; they fill no valley like a long ribbon, like a frozen cataract,—for they want *the pressure from above*, the only thing that can move them downwards into the valley.—*Description of Bear Island, vide Quarterly Journal of the Geological Society*, No. 11, p. 51.

3. *Mark cut on a rock cliff at the mean level of the Sea, with the view of determining the secular Variation of the relative level of the Sea and Land.*—My principal object in visiting Port Arthur was to afford a comparison of our standard barometer with that which

had been employed for several years by Mr Lempriere, the deputy-assistant Commissary-General, in accordance with my instructions, and also to establish a permanent mark at the zero point, or general mean level of the sea, as determined by the tidal observations which Mr Lempriere had conducted with perseverance and exactness for some time; by which means any secular variation in the relative level of the land and sea, which is known to occur on some coasts, might at any future period be detected, and its amount determined. The point chosen for this purpose was the perpendicular cliff of the small islet off Point Puer, which, being near to the tide-register, rendered the operation more simple and exact; the governor, whom I had accompanied on an official visit to the settlement, gave directions to afford Mr Lempriere every assistance of labourers he required, to have the mark cut deeply in the rock in the exact spot which his tidal observations indicated as the mean level of the ocean. The tides in the Derwent were too irregular, being influenced greatly by the prevalence of winds outside and the freshes from the interior, so that we could not ascertain with the required degree of exactness the point of mean level. It would have been desirable to have fixed a similar mark on some part of the opposite side of the island, but a prolonged series of preliminary observations of the tides are necessary; and as these had not been obtained, and our limited stay, as well as the full employment for all our observers, which the necessary experiments with the magnetometers provided, did not admit of our doing it, I can only hope that some individual with like zeal for science with Mr Lempriere, and with time at his disposal, may yet accomplish this desideratum. I may here observe, that it is not essential that the mark be made exactly at the mean level of the ocean, indeed it is more desirable that it should be rather above the reach of the highest tide: it is, however, important that it be made on some part of a solid cliff, not liable to rapid disintegration, and the exact distance above the mean level (which may also be marked more slightly) recorded on a plate of copper, well protected from the weather, by placing a flat stone with cement between, upon the plane surface or platform, which should constitute the mark from which the level of the mean tide should be measured.

The most desirable position for such another mark would be near the north-west extremity of the island, and in the vicinity of Cape Grim, near which the Van Diemen's Land Company has a small establishment.—*Sir John C. Ross's Voyage to the Southern Seas*, vol. ii. p. 22.

4. *Active Volcanoes in the Arctic and Antarctic regions.*—The earth's crust, as we approach towards the pole in the southern hemisphere, presents, in a remarkable degree, the most striking indications of the vast subterranean fires pent up within it, and, as we

now find, having vent in both the frigid zones: the volcano of Jan Mayen, actively burning within the Arctic Circle; and Mount Erebus, rising from the lofty mountain-range of the newly discovered continent of Victoria to an altitude of more than 12,000 feet above the Antarctic Ocean, and sending forth its smoke and flame to the height of 2000 feet above its crater, the centre of volcanic action in those regions of eternal snow.—*Sir James C. Ross's Voyage to the Southern Seas*, vol. ii. p. 412.

METEOROLOGY.

5. *Atmospheric Pressure under Different Latitudes.*—Our barometrical experiments appear to prove that the atmospheric pressure is considerably less at the equator than near the tropics; and to the south of the tropic of Capricorn, where it is greatest, a gradual diminution occurs as the latitude is increased, as will be seen from the following table, derived from hourly observations of the height of the column of mercury, between the 20th November 1839, and the 31st July 1843.

The mean pressure and the amount of atmospheric tide in each latitude are as follows:—

Lat.	Pressure.	Tide.	
At the Equator.	29·974	0·47	At Sea.
13°0′ S.	30·016	0·60	...
22·17	30·085	0·53	...
34·48	30·023	0·52	Cape of Good Hope and Sydney.
42·53	29·950	0·50	Van Diemen's Land.
45·0	26·664	0·31	At Sea.
49·8	29·469	0·40	Kerguelen and Auckland Islands.
51·33	29·497	0·32	Falkland Island.
54·26	29·347	0·22	At Sea.
55·52	29·306	0·27	Cape Horn.
60·0	29·114	0·24	At Sea.
66·0	29·078	0·16	...
74·0	28·928	0·16	...

The above results are arranged in belts of latitude, the observations at sea being separated from those made in harbour; this occasions more apparent irregularity than would have been the case had they been turned into two distinct tables.

It has hitherto been considered that the mean pressure of the atmosphere at the level of the sea was nearly the same in all parts of the world, as no material difference occurs between the equator and the highest northern latitudes. At Melville Island, in latitude $74\frac{3}{4}$°, it was found to be 29·870; at Igloolik, in latitude 69°, 29·770; and at Winter Island, in latitude 66° 11′, 29·798. The cause of the atmospheric pressure being so very much less in the southern than in the northern hemisphere, remains to be determined; and I trust that the very extensive series of observations made on board the Erebus and Terror, will be of material assistance in the important inquiry.—*Captain Sir James C. Ross's Voyage to the Southern Seas, during the years* 1839-43, vol. ii. p. 384. 1847.

6. *Height of the plane of Vapour in the Atmosphere, and different directions of superincumbent Streams of Air.*—As opportunities offered, experiments were made to determine the height of the plane of vapour, a desideratum of great meteorological importance, connected with all the most interesting questions regarding the distribution of aqueous vapour over the globe and the irrigation of the continents. The results of these experiments differed so widely from each other, owing chiefly to the great difficulty of any thing like exact determination in observations of this nature, and probably in some degree from an actual difference of its altitude, under various conditions of the atmosphere, ranging from one thousand two hundred to nearly three thousand feet, barely entitle them to be esteemed more than a rough approximation, giving an elevation of about two thousand feet as its mean height in the tropical regions.

On the 27th November 1839, the sky being very clear, the planet Venus was seen near the zenith, notwithstanding the brightness of the meridian sun, and was an object of much admiration to us all. It enabled us to observe the higher stratum of clouds to be moving in an exact opposite direction to that of the wind, a circumstance which is frequently recorded in our Meteorological Journal, both in the NE. and SE. trades, and has also been observed by former voyagers. Captain Basil Hall witnessed it from the summit of the Peak of Teneriffe; and Count Strzelecki, on ascending the volcanic mountain of Kirauea, in Owhyhee, reached, at 4000 feet, an elevation above that of the trade-wind, and experienced the influence of an opposite current of air, of a different hygrometric and thermometric condition; facts which tend, in some degree, to explain the means by which the equilibrium of the atmosphere, under certain conditions, is maintained in those regions. Count Strzelecki further informed me of the following seemingly anomalous circumstance,—that at the height of 6,000 feet he found the current of air blowing at right angles to both the lower strata, also of a different hygrometric and thermometric condition, but *warmer* than the interstratum.—*Captain Sir James C. Ross's Voyage to the Southern Seas*, vol. i., p. 13.

7. *Imaginary Land.*—A remarkable appearance of land was reported in the evening, and, continuing for many hours without any alteration of figure, several of the officers imagined it was really *land* they saw, assuming the appearance of many pointed hills per-

fectly covered with snow, and so calculated to deceive the inexperienced eye, that had we been prevented proceeding further, they would doubtless have asserted on our return to England that we had discovered land in this position. This appearance of land was, however, nothing more than the upper part of a cloud, marking, by a well-defined but irregular line, the limit to which vapour can ascend in these latitudes; below, is vapour in every degree of condensation; above, the clear cold space which vapour can never attain. It is always near the margin of the ice that these appearances of land are most remarkable and most deceptive. It proved a useful lesson to some of our new hands, who could not be persuaded it was not land until we had actually passed over the place of their baseless mountains.—*Sir James C. Ross's Voyage to the Southern Seas*, vol. i. p. 177.

8. *Mild Winters in England.*—An interesting communication on this subject, by Colonel Sabine, appears in the *Philosophical Magazine*. The unusual character of the winter which we have just experienced, in which the mean temperature in December, January, and February exceeded the mean temperature of the same months of 1844-45 by an average of 8 degrees, has given rise to the inquiry. The winter which, within Colonel Sabine's recollection, most nearly resembled the present, was that of 1821-1822; and undoubtedly the resemblance is in many respects very striking. The extension of the Gulf-stream in that year to the coast of Europe, instead of its terminating, as it usually does, about the meridian of the Azores, has been assigned as a cause adequate, Colonel Sabine believes, to account for the phenomena of that winter. "The warm water of the Gulf-stream spread itself beyond its usual bounds, over a space of ocean which may be roughly estimated as exceeding 600 miles in latitude and 1000 in longitude, carrying with it water several degrees higher than the temperature of the sea in ordinary years in the same parallels." The similarity of the two winters 1821-1822 and 1844-1845 having been shewn, and their agreement in those features in which they differ from ordinary winters, Colonel Sabine says, "it will naturally be asked, what evidence we have to prove or disprove an extension of the Gulf-stream in the present year, similiar to that which took place in 1821. To this it must be replied, that, strange as it may appear, this remarkable phenomenon may take place in any year without our having other knowledge of it than by its effects, although it occurs at so short a distance from our ports, from whence so many hundred vessels are continually crossing and recrossing the part of the ocean where a few simple observations with the thermometer would serve to make it known. History has recorded two instances in which the extension of the Gulf-stream is known to have taken place; and in both we owe our knowledge of it to the casual observations of an accidental voyage."

HYDROGRAPHY.

9. *Temperature of the Ocean.*—It is, therefore, evident that about this parallel of latitude, there is a belt or circle round the earth, where the mean temperature of the sea obtains throughout its entire depth, forming a boundary, or kind of neutral ground, between the two great thermic basins of the ocean. To the north of this circle the sea has become warmer than its mean temperature by reason of the sun's heat, which it has absorbed, elevating its temperature at various depths in different latitudes. So that the line of mean temperature of 39°·5, in latitude 45° S., has descended to the depth of 600 fathoms; and at the equatorial and tropical regions, this mark of the limit of the sun's influence is found at the depth of about 1200 fathoms; beneath which the ocean maintains its unvarying mean temperature of 39°·5, whilst that of the surface is about 78°.

So likewise to the south of the circle of mean temperature, we find that in the absence of an equal solar supply, the radiation of the heat of the ocean into space occasions the sea to be of a colder temperature as we advance to the south; and near the 70th degree of latitude, we find the line of mean temperature has descended to the depth of 750 fathoms; beneath which, again, to the greatest depths, the temperature of 39°·5 obtains, whilst that of the surface is 30°.

This circle of mean temperature of the Southern Ocean is a standard point in nature, which, if determined with very great accuracy, would afford to philosophers of future ages the means of ascertaining if the globe we inhabit shall have undergone any change of temperature, and to what amount, during the interval.

The experiments which our limited time and means admitted of our making, serve to shew that the mean temperature of the ocean at present is about 39°·5, or 7½° above the freezing point of pure water; and as nearly as possible the point of its greatest density. But it would be indispensable that this temperature should be ascertained to the tenth part of a degree; and as we now know where we may send any number of thermometers down to the greatest fathomable depths, without an alteration of temperature, even to that small amount, this desideratum might be very easily obtained.

These observations force upon us the conclusion, that the internal heat of the earth exercises no influence upon the temperature of the ocean, or we should not find any part in which it was equable from the surface to the great depth we have reached; a new and important fact in the physics of our globe.—*Sir James C. Ross's Voyage to the Southern Seas*, vol. ii., p. 377.

10. *Low Temperature of the Sea over Sand-Banks.**—" As no sea can be supposed absolutely motionless, the presence of a shoal, by casting up to the surface water which, but for it, would have con-

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* Sir James C. Ross's Voyage to the Southern Seas, vol. ii., p. 35.

tinued to sweep along at a greatly lower level with the general body of the current, must bring the temperature of the surface-water into nearer correspondence with that below. In low latitudes, the surface-water is hotter than that below; and, accordingly, it is a general remark, that the temperature water sinks as the water shoals, or even in *passing over banks whose depth is very considerable.* If this theory of the phenomenon be correct, the contrary ought to be observed in situations where the surface-water is colder than that below, as it is known to be, under particular circumstances, in the polar seas." The subject is one of considerable interest to the navigator, as the approach to land or shoal-water is indicated by the thermometer, in many places, with a high degree of sensibility. A remarkable instance of this kind occurs off the west coast of Africa, of which an account is given by Sir J. C. Ross.*

11. *Greatest ascertained Depth of the Ocean.*—On the 3d of June, when in latitude 15° 3′ S., and longitude 23° 14′ W., being nearly calm, and the water quite smooth, we tried for, but did not obtain, soundings with 4600 fathoms of line, or 27,600 feet. This is the greatest depth of the ocean that has yet been satisfactorily ascertained, but we have reason to believe that there are many parts of it where it is still deeper. Its determination is another desideratum in terrestrial physics of great interest and importance.—*Sir James C. Ross's Voyage to the Southern Seas*, vol. ii. p. 381.

12. *Height of Waves.*—Although the south-westerly breeze of the preceding day hardly amounted to a moderate gale, we found that this morning we had run into a heavy swell from that direction. The result of several experiments gave only twenty-two feet for the entire height of the waves, or eleven feet above and below the general level of the ocean; the velocity of the undulations eighty-nine miles per hour, and the interval between each wave nineteen hundred and ten feet.—*Sir James C. Ross's Voyage to the Southern Seas*, vol. i. p. 31.

ZOOLOGY,

13. *Marine Invertebrata found at great depths in the Ocean.*—We got soundings in three hundred fathoms, and the dredge being again put over, and allowed to trail along the bottom for two or three hours, brought up many animals, some corallines, and a quantity of sand, mud, and small stones. Amongst them we found several entirely new forms of creatures, of which accurate drawings were taken by Dr Hooker, which, together with their descriptions, are now in course of publication, and constitute one of the more interesting features of our researches. It is well known that marine invertebrate animals are more susceptible of change of temperature than land animals; indeed, they may be isothermally arranged with great accuracy. It will, however, be difficult to get naturalists and

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* Sir J. C. Ross's Voyage, vol. i., p. 34.

philosophers to believe that these fragile creatures could possibly exist at the depth of nearly two thousand fathoms below the surface; yet, as we know they can bear the pressure of one thousand fathoms, why may they not of two? We also know that several of the same species of creatures inhabit the Arctic, that we have fished up from great depths in the Antarctic Seas. The only way they could have got from the one pole to the other, must have been through the tropics; but the temperature of the sea in those regions is such that they could not exist in it, unless at a depth of nearly two thousand fathoms. At that depth they might pass from the Arctic to the Antarctic Ocean without a variation of five degrees of temperature; whilst any land animal, at the most favourable season, must experience a difference of fifty degrees, and, if in the winter, no less than one hundred and fifty degrees of Fahrenheit's thermometer—a sufficient reason why there are are neither quadrupeds, nor birds, nor land insects, common to both regions.—*Sir James C. Ross's Voyage to the Southern Seas*, vol. i. p. 207.

14. At a meeting of the Academy of Berlin on 29th October, a memoir on the larva state and metamorphoses of the Ophiuræ and Echini was communicated by M. Müller.

In 1845 M. Müller made us acquainted with some animal forms observed at the island of Heligoland; these animals he has made the subject of additional researches. The *Vexillaria flabellum* he has ascertained to be the larva of an Ascidia, probably of *Amauroncium proliferum*, Edw. But an object of greater interest, is the development and metamorphoses of an animal which, on account of its form and equivocal skeleton, has been named *Pluteus paradoxus*. From this an ophiura was produced; it thence follows that the Pluteus paradoxus is an ophiura. The earliest observations on the development of an Echinoderm are those made by M. Sars on the *Echinaster sanguinolentus* (*E. Sarsii*, Mull., Trosch.) and *Asteracanthion Mulleri*, Sars. This naturalist, whose discoveries have added many important facts to our knowledge of the transformation of the lower animals during their development, has likewise observed that the young sea-stars had no resemblance to their ulterior form. But he has given very few details respecting the internal structure of these larvæ, which doubtless proves that they are completely opaque. The larva of the Echinoderms, which form the subject of M. Muller's memoir, are so translucent that they admit of a microscopic analysis even to a size 250 times their diameter. This naturalist has published in Wiegman's Archives (1846, p. 101, tab. vi., figs. 1 and 2,) a description and figure of Pluteus paradoxus, before giving us an account of a sea-star. He does not therefore revert to the subject, but merely remarks that this larva has no connection with that described by M. Sars, unless that the animal develops itself in one direction, and that it is bilateral; but in other respects, it presents a structure so peculiar and abnormal that it could not be supposed to be the larva of an Echinoderm, even after the observations of M. Sars. Having stated this, M. Muller enters upon the description of the anatomical structure of *P. paradoxus*, and gives a multitude of details full of interest, but in which we cannot follow him.

He then passes to another class of the larva of Echinoderm which he has been able to follow to their metamorphosis, and he has ascertained, without the smallest doubt, that they belong to the genus Echinus, although he has not succeeded in determining the first stages of their development on issuing from the egg. These larvæ appear, notwithstanding the difference of their definite forms, to be modelled on the same plan as those of the Ophiuræ, and this is very apparent from the anatomical details into which M. Muller enters, details which are accompanied with very beautiful plates, indispensable to enable the reader to understand all the interesting details which the work contains.—*L'Institut*, No. 696, p. 159.

15. *On the Cranium of the Dodo in the Museum of Copenhagen. By M. Hamel.*—I have lately received, says M. Hamel, a cast of the head of the Dodo preserved in the Museum of Natural History at Copenhagen, and I have placed it in the cabinet of the Academy of Sciences of St Petersbourg. This head was first figured by Paludanus; it was afterwards, in 1651, obtained by Olearius for Duke Frederick of Schleswig-Holstein of Enkhuysen at Gottorff; it was deposited in a museum described in a work dated 1666. The objects of this museum were conveyed, under King Frederick IV., and during the war with Sweden, to Copenhagen, where this head has remained, till lately, unknown among a multitude of other objects. As I have had an opportunity of examining the head of the Dodo of Oxford, which was obtained from the Tradescant collection, and of which I possess a cast, I can compare it with the Copenhagen specimen, the only other one now known, although it wants the lower mandible.

It will be observed, in the first instance, that Olearius' descriptions of 1666 and 1674, and those recently given by M. Lehmann of Copenhagen, are insufficient to give an exact idea of the remains of a bird so remarkable. A comparison of the heads of Oxford and Copenhagen shews, that although the former be much stronger than the latter, they both agree in their various details. The Oxford head is still covered with skin, having been cut without care in 1755 and dried, while that of Copenhagen is a preparation of the bony parts, and consequently presents the structure of the bones, which is invisible in the Oxford specimen. We do not observe in the Copenhagen specimen the characteristic broadness of forehead and the angle of about 140 degrees which it forms with the beak, nor the immoderately large diameter of the whole cranium, which are so striking in the Oxford example. The latter measures from the occiput to the extremity of the beak (the gnathotheca excepted, which is nearly wanting in both) $8\frac{6}{10}$ English inches, and that of Copenhagen $8\frac{1}{10}$ inches. The greatest diameter of the Oxford specimen, including the skin which covers it, is $3\frac{9}{10}$ inches, and in the other, which is without skin, $3\frac{5}{10}$ inches.

The cast of the foot of the Dodo, which I likewise place in the Museum, is taken from the specimen in the British Museum. It is supposed to be the same that Clusius observed before 1605 in the collection of Professor Pauw at Leyden, as coming from the Island of Mauritius. As it had been announced that a living Dodo was embarked in 1598 by the Dutch, and as Clusius, in 1605, was unable to prove that an animal had been brought into the country alive, it is inferred that the bird embarked at the Mauritius in 1598 died on its way to Holland, and that it was from it the foot in Pauw's possession was obtained. The report of a Dodo having been transported from the Mauritius to Holland, has been alluded to only by De Brys, and it cannot therefore be considered as altogether authentic.

The foot preserved at Oxford, and which, as well as the head of the Dodo, is from the Tradescant museum, has been stripped of its skin by Dr Kidd, in such a manner that the bones, ligaments, and nerves, can now be studied. I have taken five different photographic views of it, which are now in the process of engraving.

In the Ashmolean Museum at Oxford, there is a large drawing of a Dodo, taken from nature, for which we are indebted to John Savery. Little attention has been hitherto paid to this. I have had a sketch carefully made and coloured like the original. This figure is particularly important on account of the feathers, wings, and tail; for the head and feet do not appear to be accurately designed. Below it there is a frog and a few cryptogamous plants which seem to be in allusion to its kind of food. This figure has been given by M. W. C. Darby to Dr Kidd, whose lectures he attended, while I examined the Ashmolean Museum in 1814, in company with the Doctor.

John Savery may have had an opportunity in 1651 of seeing a living Dodo, or at least a sketch made from nature, and by means of which he completed his figure. By a manuscript notice preserved in the British Museum, and which was obtained from L'Estrange, we learn with certainty that in the year 1638, a living Dodo was exhibited for money in London, in a house before which a figure of the bird was represented on canvass. At all events, a stuffed specimen is mentioned in 1651 in the catalogue of Tradescant's collection printed in 1656, but drawn up some years before, in 1652, by Drs Wharton and Ashmole. In this catalogue we find, at page 4, the Dodo indicated under the name of Dodar (which ought to have been Dodo-aers). It is from this specimen, conveyed in 1682 by Ashmole to Oxford, where it was destroyed by vermin, and consequently lost in 1755, that the foot and head so fortunately preserved have been obtained. It may be presumed that the Dodo which was exhibited alive in London in 1638 had been obtained after its death by the second Tradescant for the museum of his father (who died in 1638); so that the existing head and foot in the Ashmolean Museum are the same as those which L'Estrange saw in the living bird in London.

The figure of the Dodo so often cited and copied, and taken from Sloane's drawings in the British Museum (not made by Edwards, as Mr Owen affirms), does not appear to me to be taken from nature. Besides the Dodo, we there find parroquets also, and other birds or animals. Neither date nor artist are mentioned, and I consequently consider them as without value. But, on the other hand, I thought it proper to have an exact and coloured copy of a small picture (five inches in height at the most) in the Royal Museum at the Hague, representing a Dodo, which is to be seen among the well known pictures of Roeland Savery, who died in 1639, which represents Orpheus charming animals. Roeland Savery was the uncle of John Savery mentioned above. There are two other similar pictures by him, as well as others filled with animals (the Terrestrial Paradise, *Sortie de l'Arche*, &c.) which are worthy of being examined by zoologists. The living Dodo shewn in London in 1638 had very likely been conveyed to Holland, and if this individual had been observed before by R. Savery, and if it be the same that passed into the Tradescant Museum after its death, we have, from the hand of this skilful animal-painter, the general aspect, colour, &c. of the Dodo; while the head and the osseous structure, and the nerves of the feet, are seen in the Oxford specimen, of which I have brought casts, drawings, and photographic representations.

M. Hamel is about to have a model of a Dodo executed, to be painted according to R. Savery's picture, and which he means to present to the Academy as soon as completed.—*L'Institut*, No. 709, p. 252.

16. *Commercial value of Insects.*—The importance of insects, commercially speaking, is scarcely ever thought of. Great Britian does not pay less than 1,000,000 of dollars annually for the dried carcases of the tiny insect, the cochineal; and another Indian insect, which affords, by puncturing particular trees, *Lac*, is scarcely less valuable. More than 1,500,000 of human beings derive their sole support from the culture and manufacture of silk; and the silkworm alone creates an annual circulating medium of nearly 200,000,000 of dollars; 500,000 dollars are annually spent in England alone for foreign honey; at least, 10,000 cwt. of wax is imported into that country every year. Then there are the gall-nuts of commerce, used for dyeing and making ink, &c.; while the cantharides, or Spanish fly, is an absolute indispensable in materia medica.—*Boston Transcript, Athenæum*, No. 978, p. 770.

BOTANY.

17. *Esculent Funguses of England.*—The English are not a fungus-eating nation,—and in the "good old times" this might have been as much a boast as the common one, that we never eat frogs. The question is, whether we are the better or the wiser for not eating the one any more than the other. Though we do not eat frogs like our neighbours the French, we are rather celebrated for our love of another of the reptilian family, turtle. There is no reason why we should eschew frogs, and relish turtle. There is still less for our eating one or two of the numberless edible funguses which our island produces, and condemning all the rest as worse than useless, under the name of "toad-stools." It is not so on the continent of Europe; where very generally the various species of fungi are esteemed agreeable and important articles of diet. The great drawback on the use of these esculents in this country is, that some are poisonous, and few persons possess the skill to distinguish them, with the exception of one or two species, from those which are edible. In the markets at Rome, there is an "inspector of funguses," versed in botany, and whose duty it is to examine and report on all such plants exposed for sale. The safety with which these vegetables may be eaten, has led to a very large consumption in that city, where not less than 140,000 pounds weight, worth L.4000 sterling, are annually consumed. This in a population of 156,000 souls. We cannot estimate the value of funguses in our own country for an article of diet as less than in Italy; nor believe that the supply would be in a less ratio. If this be correct, the value of the funguses which are allowed to spring up and die, wasted in Great Britain, would be about half a million sterling in each year.—*Athenæum*, No. 1037, p. 948.

NEW PUBLICATIONS RECEIVED.

1. A Voyage of Discovery and Research in the Southern and Antarctic Regions during the years 1839-43. By Captain Sir James Clark Ross, R.N. With Plates, Maps, and Woodcuts. In 2 vols. 8vo. London: John Murray, Albemarle Street. 1847.

2. Illustrations of Instinct, deduced from the Habits of British Animals. By J. Couch, F.L.S., &c. London: John Van Voorst. 1847.

4. Lehrbuch der Chemischen und Physikalischen Geologie. Von Dr G. Bischof. Bonn, 1847. Vol. 1.

4. Lehrbuch der Physikalischen Geographie und Geologie. 8vo. Bern.

5. Professor Ritter of Berlin, on the Geographical distribution of some characteristic productions of Arabia, viz., Coffee-Tree, Camel, and Date Palm. 8vo, pp. 326.

6. Dr Spittal's Introductory Discourse on Pathology and the Practice of Medicine. 1847.

7. On the Parallel Roads of Lochaber; with remarks on the change of Relative Levels of Sea and Land in Scotland, and the Detrital Deposits of that Country. By David Milne, Esq., F.R.S.E., &c. 4to. Neill and Company, Edinburgh. 1847.

8. Tafeln zur Bestimmung der Mineralien. Von Franz v Kobell. 4^{te}, Auflage. 1846.

9. Monographie General de la Famille, des Plantaginès, par F. Marius Barnioud. Paris. 1846. 4to.

10. Berichte über die Mittheilungen von Freunden der Naturwissenschaften in Wien. Von Wm. Haidinger. Wien. 1847.

11. Mittheilungen der Naturforschenden Geselleschaften Bern. 1847.

12. Vereinfachung der Lehre von den Gängen von J. Fournet. Ubersezt Von Muller & Cotta. 8vo, pp. 118. Freyberg, 1846.

⁂ *Remarks in next Number of Journal.*

List of Patents granted for Scotland, from 21*st June to* 21*st September* 1847.

1. To William Darling, of Glasgow, Scotland, ironmonger, "improvements in moulding, and in the manufacture of certain articles of cast-iron."—23d June 1847.

2. To Thomas Craddock, of Birmingham, in the county of Warwick, engineer, "improvements in steam-engines and boilers, and in machinery connected therewith."—23d June 1847.

3. To Clemence Augustus Kurtz, of Manchester, in the county of Lancaster, manufacturing chemist, "certain improvements in the mode of preparing and using indigo in the dyeing and printing of woollen, cotton, and other fabrics."—29th June 1847.

4. To Clemence Augustus Kurtz, of Manchester, in the county of Lancaster, manufacturing chemist, "a new manufacture of a certain colouring matter to be used in the dyeing or in the printing of woollen, cotton, silk, and other fabrics."—29th June 1847.

5. To William Knowelden, of Great Guildford Street, Southwark, engineer, "improvements in steam-engines."—30th June 1847.

6. To John Shaw, of Blackburn, in the county of Lancaster, manager, "certain improvements in machinery or apparatus for carding, drawing, slubbing, and roving cotton, wool, and other fibrous substances."—2d July 1847.

7. To Paul Gilbert Prelier, of Rue de Rivoli, Paris, in the kingdom of France, gentleman, being a communication from abroad, "im-

provements in the manufacture of dry sulphuric acid, and in the manufacture of smoking or Nordhausen sulphuric acid."—9th July 1847.

8. To George Winslow, now of Boston, in the State of Massachusetts and United States of America, merchant, but lately of Burton Crescent, in the county of Middlesex, being a communication from abroad, "improvements in machinery for manufacturing files and rasps."—12th July 1847.

9. To John Law, of York Place, Portman Square, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in yarns, and in the machinery by which the same are manufactured."—13th July 1847.

10. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, being a communication from abroad, "improvements in the manufacture of screws."—19th July 1847.

11. To Henry John Nicoll, of No. 114 Regent Street, in the county of Middlesex, tailor, "improvements in garments, and in pockets, bags, and other receptacles."—19th July 1847.

12. To Edward Slaughter, of Avon Side Iron-Works, Bristol, engineer, "improvements in locomotive engines."—20th July 1847.

13. To William Broadbent, of Manchester, and of Little Lever, both in the county of Lancaster, paper manufacturer and dealer, "improvements in the manufacture of paper."—21st July 1847.

14. To James Johnstone, of Willow Park, Greenock, Esquire, "certain improvements in the manufacture of sugar."—21st July 1847.

15. To Egbert Hedge, formerly residing at No. 7 Howard Street, in the parish of St Clements Danes, and county of Middlesex, now residing at the Commercial Hotel, Blackfriars Road, in the county of Surrey, gentleman, "certain improvements in rails for railways, and in the manner of securing them."—22d July 1847.

16. To William Breynton, of the Inner Temple, in the city of London, Esquire, "certain improvements in rotatory steam-engines, and in the means of working the same."—21st July 1847.

17. To John Yule, of Sauchiehall Street, in the city of Glasgow, North Britain, "certain improvements in chairs used on railways, and in the fixing of the same."—27th July 1847.

18. To Henry Mapple, William Brown, and James Lodge Mapple, of Child's Hill, in the parish of Henden, "improvements in communicating intelligence by means of electricity, and in apparatus relating thereto, part of which improvements are also applicable to other purposes."—2d August 1847.

19. To Moses Poole, of the Patent Office, London, gentleman, being a communication from abroad, "improvements in the manufacture of cast-metal, iron, and steel."—5th August 1847.

20. To Robert Stirling Newall, of Gateshead, Esquire, "certain improvements in locomotive engines."—6th August 1847.

21. To John Macintosh, of Bedford Square, in the county of Middle-

sex, gentleman, "improvements in engines to be worked by steam or other suitable fluid, and improvements in propelling carriages and vessels."—6th August 1847.

22. To William Thomas, of 129 Cheapside, in the city of London, merchant, being a communication from abroad, "certain improvements in stays, which improvements are applicable to other useful purposes."—11th August 1847.

23. To Pierre Armand le Comte de Fontainemoreau, of No. 4 South Street, Finsbury, London, English and Foreign Patent Office for Inventions, being a communication from abroad, "certain improvements in the machinery for cutting wood, and in laying and uniting veneers."—11th August 1847.

24. To James Webster, of Sneinton, in the county of Nottingham, "an improved atmospheric buffer to be applied to railway carriages and other vehicles travelling on railways."—12th August 1847.

25. To John Thompson Carter, of Drogheda, in the county of the town of Drogheda, in Ireland, flax-spinner, "improvements in machinery for crushing, bruising, and preparing flax, hemp, and other fibrous materials requiring such treatment."—16th August 1847.

26. To Frederick Steiner, of Hyndburn Cottage, near Accrington, in the county of Lancaster, turkey-red dyer, being a communication from abroad, "improvements in the manufacture of sugar."—16th August 1847.

27. To Thomas Birchall, of Ribbleton Hall, in the county of Lancaster, gentleman, "improvements in folding newspapers, and other papers."—16th August 1847.

28. To James Morrison, of Paisley, in the county of Renfrew, in Scotland, shawl-manufacturer, "improvements in applying power in propelling or moving carriages, and in giving motion to machinery."—18th August 1847.

29. To Philip Henry Holland, of Chorlton-upon-Medlock, in the borough of Manchester, in the county of Lancaster, surgeon, being a communication from abroad, "improvements in applying manure to land."—20th August 1847.

30. To John Blyth and Alfred Blyth, both of St Anns, Limehouse, in the county of Middlesex, engineers, and John M'Culloch of Mosemore Cottages, Old Kent Road, in the county of Surrey, chemist, "certain improvements in apparatus for distilling and rectifying."—23d August 1847.

31. To Stephen White, of No. 30 Winchester Row, New Road, in the parish of Mary-le-bone, in the county of Middlesex, clerk in the employ of the Great Western Railway Company, "a new means of producing gas, both as to apparatus and materials from which the gas is produced."—25th August 1847.

32. To Charles Hancock, of Brompton, in the county of Middlesex, gentleman, "improvements in the preparation of gutta percha, and in the application thereof, alone and in combination with other materials, to manufacturing purposes, which improvements are also applicable to other substances."—26th August 1847.

33. To Pierre Armand le Comte de Fontainemoreau, of No. 4 South Street, Finsbury, London, English and Foreign Patent Office for Inventions, being a communication from abroad, "certain improvements in steam and other engines."—31st August 1847.

34. To Edward Manlove, of Nottingham Park, in the county of Nottingham, gentleman, being a communication, "certain improvements in propelling."—31st August 1847.

35. To William Pidding, of Alfred Place, Bedford Square, in the county of Middlesex, gentleman, "an improved process, or improved processes, for preparing certain vegetable extracts, and also for preserving the aroma of certain vegetable substances from the atmosphere."—1st September 1847.

36. To Charles Chinnock, of Seymore Place, Little Chelsea, in the county of Middlesex, gentleman, "improvements in regulating motion, and controlling friction in the joints, and other parts of furniture, machinery, and carriages."—3d September 1847.

37. To Arthur Harry Johnson, of Gresham Street, in the city of London, assayer, "improvement in refining silver-lead, by effecting a saving in one of the materials used."—6th September 1847.

38. To Jules Jean Baptiste Martin de Lignac, of Portland Street, in the county of Middlesex, gentleman, "improvements in preserving milk."—8th September 1847.

39. To Pierre Armand le Comte de Fontainemoreau, of 4 South Street, Finsbury, English and Foreign Patent Office for Inventions, being a communication from abroad, "certain improvements in machinery for preparing cotton, and other fibrous substances."—13th September 1847.

40. To Baron Charles Wetterstedt, of Rhodes Well Road, Limehouse, in the county of Middlesex, "improvements in the manufacture of sheet-metal for sheathing and other purposes, in preventing the corrosion of metal, and in preserving wood and other materials."—15th September 1847.

41. To William Bacon and Thomas Dixon, both of Bury, in the county of Lancashire, engineers, "certain improvements in steam-engines."—17th September 1847.

42. To Kasimer Vogel, of St Paul's Churchyard, London, gentleman, "a new manufacture of weavers' harness, and machinery for the production of the same."—17th September 1847.

43. To Robert Wilson, of Low Moon Iron Works, Bradford, in the county of York, engineer, "certain improvements in machinery, and arrangement thereof for forging, stamping, punching, cutting, and pressing metals, and other substances."—17th September 1847.

44. To John Mollett, of Austin Friars' Passage, in the city of London, merchant, being a communication from abroad, "certain improvements in fire-arms, and in cartridges."—21st September 1847.

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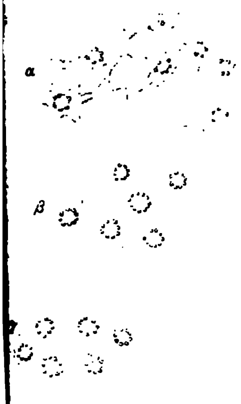

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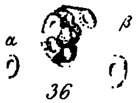

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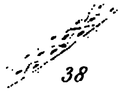

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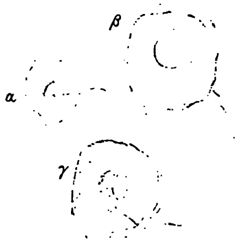

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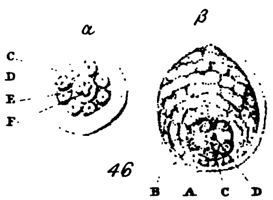

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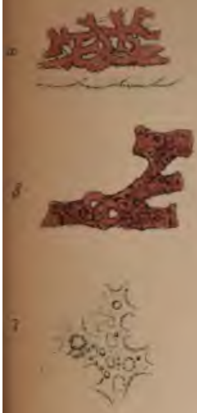

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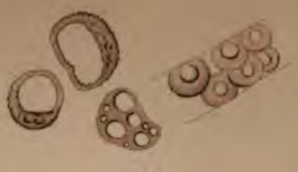

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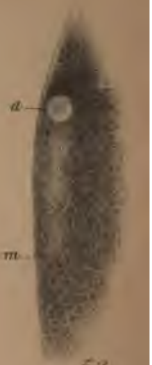

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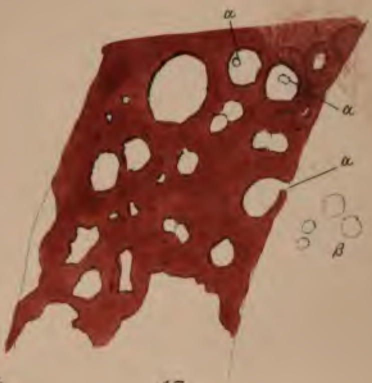

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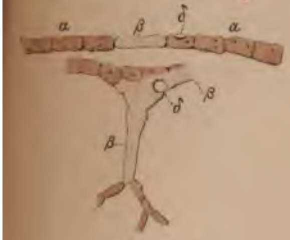

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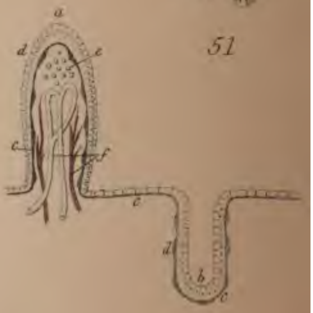

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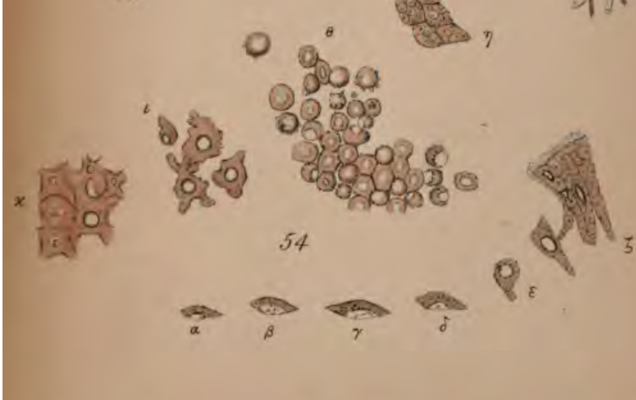

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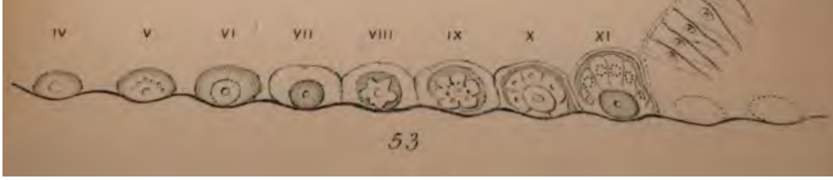

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